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Papers on Hydraulic Engineering. By SAMUEL McELROY, C. E.

(Continued from p. 19.)

No. 2.—DISTRIBUTION.

As a consequence of the use of water supply under pressure, lines of distribution service to the several points of consumption are provided, and are common in principle of use to both the intermittent and constant systems of service, which have been previously noticed.

From the reservoirs of storage, or points of supply by stand-pipes and otherwise, these lines, branching off in various directions, with various proportions of calibre, and differences in material, have been applied to the solution of the problem of adequate delivery, adequate strength, and protection from the several contingencies of use; and have, therefore, involved not only the question of serious expenditure, but various interesting questions of propriety in manner of construction, arrangement, and method of use.

A system of distribution comprises lines of feeding mains, service mains, special service pipes, and the various appurtenances connected with the details of delivery, and is controlled in character by the extent of service as to relative supply and demand, the conditions of service as to pressure, and by range of admissible expenditure.

Distribution is generally the most costly feature of any water supply. Its proportion, for instance, at Brooklyn, in an original contract total of $4,200,000, was not less than $1,750,000; and while this

matter of cost is determined by the extent of the system, the latter, in turn, chiefly determines the item of annual income, thereby regulating in an important way the propriety of this extent.

The materials which have been used at various periods and in various localities, and which may be noticed under a general head, are stone and brick conduits, earthenware pipes and pipes of glass, bituminized paper, lead, wrought iron, cast iron, zinc, tin, and other metals, wood, and other materials, some of which will be discussed under special heads.

Arrangement.—The most perfect system in this respect is that which transmits the supply to its consumers with the least loss in pressure from frictional and other obstructions to its flow.

To this end, a subdivision in supply districts is requisite for any system, properly controlled by lines of feeding mains, of adequate calibre for free delivery and circulation; and for want of proper attention to this principle, the value of many supplies has been seriously injured, under circumstances which render remedies difficult in application.

In a number of cases, we find small and independent feeding mains, attached to service mains as low in calibre as two, three, and four inches. Not only does it result from this, that the pressure on the house service is seriously vitiated, from a natural law in hydrodynamics, but in the important special use of a fire supply, instances abound where serious losses have resulted from a restricted discharge at the hydrants on this account. And as experience proves that a line of 4-inch pipe will not properly supply more than one fire engine, except under an unusual pressure, it may be taken for granted, wherever fire supply is an object, that all lines of this calibre should be short, and connected with larger mains, and that 6-inch mains should be assumed as the minimum for lines of general service. This principle was adopted as a rule in the arrangement of the Brooklyn distribution, the use of 4-inch pipe being restricted to hydrant branches. It is also adopted by the Croton Board for extensions of service.

Due attention to this arrangement of districts improves the operation and efficiency of the supply, admits greater extensions with less loss in head, and a much more simple and effective location of stop-cocks, which control delivery, accidents, and repairs, and which need, for this reason, particular care. The larger and more complete this framework of feeding mains, around the special districts of service mains, the better, within limits of reasonable cost, and subject to local contingences of supply, pressure, demand, and extension; and for these, as thus determined, the calibres may range from eight, ten, and twelve inches, to sixteen, twenty, twenty-four, and thirty. Some of the Croton mains of this class are thirty-six inches in diameter. As an illustration of forethought on this subject, and a good example, the following extract is made from the Philadelphia Water Works Report of April 19, 1855:

"The distribution of the old city proper, although arranged in 1819, before any other in this country, is very perfect, the system originally laid down having been strictly adhered to."

"The city is crossed upon the highest streets, running north and south, by two supply mains of twenty

inches diameter each; and in the opposite direction, through the centre street, runs a main of thirty inches diameter; the whole plot is circumscribed by mains of sixteen inches diameter, reducing to twelve and ten inches, and it is crossed north and south at intervals of about 1200 feet by mains of ten and twelve inches; these again are crossed east and west by several mains of sixteen, twelve, and ten inches diameter (all connected at proper intervals, and making a complete net-work of mains and feeders), supply the remainder of the distribution, which consists of six, four, and three inch pipes. The arrangement is quite complete, the only source of regret being that any pipes so small as three inches should have been used; but at the very early date when the distribution was devised and commenced, they were considered more than sufficient. Many new water works, however, executed within a very short period, and long after those of the old city, have, notwithstanding the advice and experience of Fairmount Works, fallen into the same error, and are now suffering from the want of larger pipes and mains."

Usual Practice in Material.—Notwithstanding the varieties of material which may be and have been applied to water distribution, the general modern practice has adopted the use of cast iron mains for service. For house service pipes lead is generally used. These materials have several advantages and defects, which require special notice.

Cast Iron Pipes.—The strength and durability of this material; its facility for ready adaptation to the different forms of mains and their special appurtenances; facility in making joints, and in tapping for house service; in connexion with the influence of long established custom, comprise its chief advantages.

The ordinary form of joint in use, the "spigot and faucet," is that made with a hub at one end of the pipe, expanded to take another pipe, with a joint for gasket and lead packing, varying in width from three-eighths of an inch upwards, and averaging about six inches in length. These admit considerable expansion and contraction, and are therefore preferred to flanch joints, or to the conical joints originally introduced in London, and abandoned on account of frequent breakage from this cause.

Special cases, however, frequently occur, where flanch joints may be used with economy, and fully guarded against objectionable rigidity.

It is found, however, that expansion affects the lead joint sensibly, causing it to "crawl" in its socket, particularly after the gasket is destroyed, to remedy which in part, the hubs are cast with a countersunk bead, which tends to hold the lead in place.

In pipe lengths and sizes, improvements have been gradually made in foundry work, increasing the lengths from the old thirty inch sections of the Albany and other works, up to ordinary lengths of nine feet, and more recent lengths of twelve and fifteen feet, with much advantage in handling and economy in laying, since the expense at the joints is a serious item of cost, the lead alone, independent of labor, ranging in value from about 58 cents per joint for 6-inch pipe to $5·18 for 36-inch. Practically, at the foundries, there is but a slight difference in cost per ton between the shorter and longer lengths, except for very large diameters.

The usual test is that of 300 lbs. per square inch under hydraulic pressure, in connexion with a hammer test, which may be made needlessly severe, with a pipe under great tension. This is equivalent to a hydrostatic head of 690 feet, or about four times the usual pressure in water works practice—245 feet, as at Albany, when the upper district is connected with the lower, and 230 feet, as at Watertown, and a few similar cases, being exceptional instances in the United States,

although exceeded in some European supplies. This test, however, has this fallacy, that it is applied only to the pipe castings, while the joints, as the true source of leakage, are laid without any trial, except that of actual use. There is an extreme and hurtful zeal in one case, with such a peculiar metal as cast iron, which may be permanently injured before it shows any symptoms of yielding, and an oversight in the other extreme, as to the ordinary points of damage. Unquestionably, a considerable part of the loss of supply now attributed to house waste, is due to defective joints in the service.

Objections.—The objections to the use of cast iron may be summed up in its cost, its defects in casting, its loss of length and other defects at the joints, the difficulty in breaking joints, and its liability to destruction and discoloration of supply by oxides, and by tuberculation, which also restricts its delivery.

Cost.—The excessive weight of metal used for the qualities in ordinary application, involves items of expense for transporting and handling material, freight, distributing on line of work, and labor in laying, as well as for value of material. To this is added the cost of jointing. The weights for pipes of 12 feet length, may vary from 225 lbs. for 4 inches diameter to 4600 for 36 inches diameter, and the following table will show the variation in standard, from two recent water works schedules :—

Cast Iron Pipes.—Standard Weight.

Length and Class.	4 in.	6 in.	8 in.	10 in.	12 in.	16 in.	20 in.	24 in.	30 in.	36 in.
9 feet A, .		330	430		680		1600		3000	3600
9 " B, .		360	500		820		1900		3700	
9½ " A, .		347	453		716		1682		3160	
9½ " B, .		378	525		864		2000		3900	
12 " A, .					920		2100		3960	4750
12 " B, .					1080		2500		4890	
12 " (to lay)	225	368	480	750		1132		2511	3845	4607

In this case the A class is arranged for a practical head of about 100 feet, and the B class of about 150 feet, the lengths including the hubs; the weights given in the last line refer to a head of about 120 feet, and the length is exclusive of hubs.

Including the value of the material, the cost of such pipes per lineal foot laid, may vary from about 55 cents for 4-inch to about $9·50 for 36-inch, and readily aggregates a formidable sum.

This item can only be modified in amount by any substitutes, and must always keep the distribution service in the front rank of expense. We may have occasion to allude to these modifications under another head.

Casting—In foundry work, this metal is liable to defects from various causes. Unequal shrinkage, uneven thickness, blisters, sand holes, air cells, cold shuts, &c., may result from the spring of core bars, yielding at the ends, displacement of core coating, unequal rate of cooling, steam and air bubbles, the effects of local ebullition, and of floating sand, slag, dirt, &c., on the hot metal. For these and other

obvious reasons in grade of metal, pipe castings should be made vertically, hub down, in loam moulds, and of a pure quality of iron. But their quality is generally inferior, although vertical castings are coming more fully into use.

Soft and impure iron is less strong and much more subject to oxidation and tuberculation, than better grades, although the relative cost of material operates strongly in favor of its use for water works by contractors, while the same consideration favors green sand castings in inclined and horizontal moulds.

Joints—By the common method of jointing described, it follows that an increase in weight is required for the hubs, both on account of their enlargement, and for additional strength to resist the process of caulking; and whatever length of pipe is appropriated to the joint is a direct loss, varying from five to seven inches on each pipe. Economy in movement and in jointing, therefore, urges the adoption of the longest practicable lengths. It is also evident, that reduction in number of joints must reduce the contingencies of leakage, so far as affected by the operation of expansion, contraction, or other general causes.

When it becomes necessary to take up a line of pipe, or a defective pipe, the lead must be melted by a hot fire from several hubs, to spring out one piece. This process is objectionable for several reasons, but is inevitable with this manner of jointing.

Practically, all the tightness of a lead joint is included in the depth of an inch or so, which is expanded by the action of the caulking tool, the gasket serving no other purpose than a mould for the running lead, and frequently defective in this office. There is room then for question, whether the sleeves which are frequently used for jointing large pipes may not be commonly applied with advantage, since they may be easily and cheaply cast, save the losses in length at the hubs, are jointed with double-caulked faces, and may be cut away and replaced without the trouble, expense, and risk of a hot fire on the mains for several lengths. It may also be said that flanch joints save the losses in length, and as "face" or as "rust" joints may be made tight and durable. They are also cheaper, altogether, than hub joints, and are frequently used on large mains for special connexions. With occasional "slip joints," or with occasional pipes or flanches cast to admit expansion, their chief objection may be obviated.

The most perfect hub joint is that which is made with a full lead packing, caulked both on the outside and inside. This secures a smooth water-way, avoiding the use of gaskets, but can only be applied with convenience and certainty to pipes of large calibre. The 36-inch force mains of the Brooklyn pumping engines have 7-inch hubs, jointed in this way. In jointing, as in all other mechanical operations, good workmanship is essential to any method which may be adopted.

Oxidation.—The natural action of water on cast iron pipes produces oxidation. It has been held that this effect is mainly controlled by the constituents of the water in contact with the iron, different qualities having different effects; but it is much more certain that the

constituents of the iron determine the rapidity and extent of oxidation in connexion with those of the water, and as the most prominent cause. A walk through any pipe yard after a rain, very clearly shows how much more susceptible to this action some castings may be than others, probably from the same foundry; and it is evident, without argument in detail, that the differences in combination of material, and in method of casting, must essentially affect the liability to this action, and particularly with a class of castings which are impure, and rudely manufactured, at comparatively low prices.

From the thickness of pipe which the quality of metal, rather than the limit of resistance, renders obligatory in the foundry, the ordinary process of oxidation involves no immediate danger to the castings under pressure. Its chief objection is found in the impregnation and discoloration of the contents of the mains. Where the house-taps are placed on the top of the mains, drawing their supply from the purest currents, and the supply is not subject to extraordinary agitation, oxidation may progress to a large extent, without making itself apparent in the house service; but if the hydrants are thrown open for fire purposes, or other causes of general agitation occur, disturbing the heavier particles in the mains, the evidences of their presence become at once and prominently sensible, and in some cases render the supply unfit for domestic use for a length of time. With unprotected mains, this is an evil which can be remedied only in degree, as to its palpable effects, while the process of destruction is as certain as it is slow. Unquestionably, different kinds of water will differ in degree of effect in oxidation, and different proportions of air in the same water have also their degrees of effect; but the prominent cause is attributable to the castings themselves, as being more or less impure, and therefore more or less subject to oxidation.

(To be Continued.)

The Economic Construction of Girders.

From the Lond. Civ. Eng. and Arch. Journal, May, 1862.

(Continued from page 88.)

Calculations of the Weight of Iron in the Booms.—The lengths of the bays, except in the second form, are all equal to unity. In figs. 2 and 3, it may be observed that in certain cases, the first bay of one of the booms may be omitted along with the end pillar: two additional combinations thus produced will be noted in the table of final results.

It is impossible to make the estimates of the booms with minute accuracy, as for this purpose the nature of every detail of construction would have to be specified. All we can attempt is to give values to the factor a just so great as to cover liberally all extras to which judiciously-designed girders may be liable. Thus, for instance, the boom at the level of which the roadway is situated, will generally demand an excess of material to counterbalance any cross strains brought upon it, or augmentations of its longitudinal stresses from the action of the principal horizontal bracing; but no special change in the values of a for the boom on account of this will be made in the calculations; it will be understood that to meet it a small portion

of the values of a may be abstracted from the one boom and transferred to the other, the latter being the one at the level of the road-way.

TABLE X.

1st Form of Girder. Estimate of the Weight of the Booms when the load is all at the upper level.

Upper Boom.	a.	Stress.	Weight.
1st Bay	·00180	4·5	·00810
2	·00070	13·5	·00945
3	·00060	19·5	·01170
4	·00055	22·5	·01238
			.04163
Lower Boom.			
1st Bay	·00067	6·0	·00402
2	·00045	15·0	·00675
3	·00045	21·0	·00945
4	·00045	24·0	·01080
			·03102

TABLE XI.

1st Form, with the load at the lower level.

Upper Boom.	a.	Stress.	Weight.
1st Bay	·00140	6·0	·00840
2	·00070	15·0	·01050
3	·00060	21·0	·01260
4	·00055	24·0	·01320
			·04470
Lower Boom.			
1st Bay	·00080	4·5	·00360
2	·00045	13·5	·00608
3	·00045	19·5	·00877
4	·00045	22·5	·01012
			·02857

TABLE XII.

2d Form, with the load at the upper level.

Upper Boom.	a.	Stress.	Length.	Weight.
1st Bay	·00170	5·3838	1	·00915
2	·00070	15·1707	1	·01062
3	·00060	20·7869	1	·01247
4	·00055	23·0000	1	·01375
				·04599
Lower Boom.				
1st Bay	·00067	6·0	1·1964	·00481
2	·00045	15·0	1·0659	·00720
3	·00045	21·0	1·0710	·01012
4	·00045	24·0	0·6667	·00720
				·02933

TABLE XIII.

2d Form, with the load at the lower level.

Upper Boom.	*a*.	Stress.	Length.	Weight.
1st Bay	·00140	6·0	0·8182	·00687
2	·00070	15·0	0·9256	·00972
3	·00060	21·0	0·9229	·01163
4	·00055	24·0	1·3333	·01760
				·04582
Lower Boom.				
1st Bay	·00095	3·6819	1	·00350
2	·00045	11·9133	1	·00536
3	·00045	18·2314	1	·00820
4	·00045	22·0000	1	·00990
				·02696

Had the above two tables been calculated with one uniform value of *a* for both booms, the total results would have been the same as for the regular form similarly calculated.

TABLE XIV.

3d Form. The estimates of the Booms for the two cases of a load at the upper and at the lower levels, may be made alike for this girder.

Upper Boom.	*a*.	Stress.	Weight.
1st Bay	·00081	10·5	·00850
2	·00065	18·0	·01170
3	·00058	22·5	·01305
4	·00055	24·0	·01320
			·04645
Lower Boom.			
1st Bay	∞	0·0	·00360
2	·00045	10·5	·00473
3	·00045	18·0	·00810
4	·00045	22·5	·01012
			·02655

TABLE XV.

4th Form. Estimate of the Booms for both cases of the load being at the upper and at the lower levels.

Upper Boom.	*a*.	Stress.	Weight.
1st Bay	∞	0·0	·00800
2	·00080	10·5	·00840
3	·00060	18·0	·01080
4	·00055	22·5	·01238
			·03958
Lower Boom.			
1st Bay	·00045	10·5	·00473
2	·00045	18·0	·00810
3	·00045	22·5	·01012
4	·00045	24·0	·01080
			·03375

TABLE XVI.

SUMMARY OF RESULTS FOR THE BOOMS.

	Top.	Bottom.	Total.
1st Case: Load at Top.			
1st Form of Girder, . .	·04163	·03102	·07265
2d do., .	·04599	·02933	·07532
3d do., . .	·04645	·02655	·07300
4th do., .	·03958	·03375	·07333
2d Case: Load at Bottom.			
1st Form of Girder, . .	·04470	·02857	·07327
2d do., .	·04582	·02696	·07271
3d do., . .	·04645	·02655	·07300
4th do., .	·03958	·03375	·07333

TABLE XVII.

GENERAL SUMMARY OF RESULTS FOR THE GIRDERS COMPLETE.

(Composed from Tables IX. and XVI., &c.)

	Without end pillars.	With end pillars as ties.	With end pillars as struts.
1st Case: Load at Top.			
1st Form, fig. 1, . .	·10566	·10836	·10926
2d " " .	·10756	·11026	·11116
3d " fig. 2, . .	·11373	·11448	·12093
4th " fig. 3, .	·11730	·12203	·11910
2d Case: Load at Bottom.			
1st Form, fig. 1, . .	·10423	·10693	·10783
2d " " .	·10302	·10572	·10662
3d " fig. 2, . .	·10933	·11023	·11563
4th " fig. 3, .	·12097	·12637	·12297

3d Form, with load at top, and with end pillar and 1st bay of bottom boom omitted, = 0·11013.

4th Form, with load at bottom, and with end pillar and 1st bay of top boom omitted, = 0·10297.

This table of results offers many interesting and instructive points worthy of attention, but we will only say a few words thereon. The most remarkable general conclusion is, that the weights vary in a much less degree than could have been expected; this arises from the ratios of table IX. being in a great measure overborne by the reverse action of those in table XVI. We must not, however, be misled by this into undervaluing the importance of a consideration of the inclinations of the braces, for were the spans in proportion to the depths much increased, we should get other results.

The most important arrangement is that wherein the end pillar acts as a strut; and for this, when the load is at the lower level, the re-

sults are, as regards economy, in the order in which we would have expected; when, however, the load is at the upper level, which will generally form the cheapest arrangement for a bridge, it appears that the regular form, fig. 1, ranks first in order of economy. With the end pillars as struts, both the heaviest and lightest constructions occur when the load is at the lowest level, the heaviest, or that for fig. 4, being 15·33 per cent. in excess of the 2d form; when the load is at the upper level, the heaviest, No. 3, exceeds the lightest, No. 1, by 10·7 per cent.

It should be noted, however, that the absolutely most economical structure is not included in the table; but it is obvious that the results for it would differ very slightly indeed from the least of those for the first two forms, and from this and constructive reasons, the first or regular form, fig. 1, should always be preferred.

That modification of the fourth form in which the end bays of the top boom and the end pillars are done away with, appears at first sight to possess economic merits, but it would demand a considerable addition of material to be devoted to steadying the top boom in a lateral direction.

We will now show the use of the tables in calculating the actual weights, in tons, of any girder, suited to bear a working load consisting of one ton movable and half a ton fixed per foot run of the span.

The unit of length is $= s \div 8$ feet, and the unit of weight or stress is $= 1\frac{1}{2}\, s \div 24$ tons; and as the quantities in the tables are calculated for the halves of the lengths of the girders, we have for the whole weight—

$$\text{Weight of girder} = \text{tabular number} \times \frac{s}{8} \times \frac{1\frac{1}{2}\, s}{24} \times 2 =$$

$$\text{Tabular number} \cdot \left(\frac{s}{8}\right)^2 \text{ or} = \text{tabular number} \cdot D^2.$$

Let us take examples of the regular form, fig. 1, with end struts and the load at top. The tabular number for these will be $= 0{\cdot}10926$, and we have

Span for Calculation.	Weight of Girder complete.
120 feet	24·58 tons.
130 "	28·85 "
140 "	33·46 "
150 "	38·41 "
160 "	43·70 "

These weights will appear to many engineers to be very light for such spans (the girders being *double ones*, or each suited to carry a whole line of railway); this lightness arises from the depth of structure. To show that there is really an excess of strength, we may calculate the stress per inch of section in the various parts. The weight of a prism of good rolled iron, one foot long and one inch in sectional area, weighs very nearly 0·0015 tons, and as a in the tables represents the weight of that fraction of such a bar required to convey one ton of stress, the number of tons of stress per sectional inch

of the part will be $= 0{\cdot}0015 \div a$. Now, applying this formula, we get the following stresses per sectional inch, on the supposition that all the material of any bay or brace is available as though spread over the calculated length.

Upper Boom.	Stress in tons.	Brace.	Stress in tons.
Bay 1	0·833 Comp.		
2	2·143 "	A	2·5 Comp.
3	2·500 "	B	2·239 "
4	2·727 "	C	1·948 "
		D	1·667 "
Lower Boom.		E	1·250 "
Bay 1	2·239 Tensive.	F	3·333 Tensive.
2	3·333 "	G	3·333 "
3	3·333 "	H	3·333 "
4	3·333 "		

To arrive at the actually efficient sections, and the corresponding stresses per sectional inch of metal, certain per centages would have to be deducted from the values of a, given in tables I. and X. These deductions would be much less in the case of struts than for the ties, on account of less weakening from rivets, and less material required for joint-plates. And it will hence be seen that the strength given to the struts is very considerably in excess of that very frequently, but the writer thinks, with danger, allotted to them.

The gross weight of the ties in proportion to the weight of the efficient portion depends so much upon the lengths of the plates used, that no satisfactory rule can be given for estimating the extras; the two principal items are—1st, the part of the section cut through or away by the rivet holes; and 2d, the joint-plates. Now we may readily diminish one of these, but by increasing the other, so that, for plates of a given length, there is some degree of uniformity in the sum of these two additions of, in a certain sense, useless material. As an approximation, we may estimate the various portions of a thus:—

Solid metal in section of tie	= 100	or 69·0
Metal rendered useless by rivet holes	= 25	" 17·2
Extra metal in joint-plates, rivets, extra length, &c.	= 20	" 13·8
	145	100·0

In the case of the bays of the lower boom, there will be no extra length, but long joint-plates; in that of the diagonal ties, there may be no joint-plates, but the length may exceed that used in the calculations. According to the above estimate, we should take only 69 per cent. of a, in calculating the true stress per solid inch of section: thus, when $a = 0{\cdot}00045$, the stress so counted will be $= 0{\cdot}0015 \div 0{\cdot}69\,a = 4{\cdot}83$ tons.

For a double girder with a span for calculation equal to 144 feet, the weight is 35·4 tons, or in detail as follows:—

		Tons.	Per cent. of whole.		Approx. Ratios.	
Top of Girder	=	13·488	or	38·10	4	2
Bottom of ditto	=	10·050	"	28·39	3	
Web and Pillars	=	11·862	"	33·51	3·5	1

Here we find that when the depth of our open-work girder is made one-eighth of the span, the weight of the web complete is just one-half of that of the booms taken together, or one-third of the whole. If the depth be made only half of this, or one-sixteenth of the span, the weight of the booms may be roughly estimated at double, or say = 4; whereas, as explained in a former paper, the weight of the web would not be materially altered—that is, it will still remain nearly equal to 1, and the weight of the whole girder will be represented by 5. Now, in the best examples of plate girders constructed with a depth equal to one-sixteenth of the span, the booms may be set down at the same as for an open-work girder of equal depth,—that is, their weight will be represented by 4, while the web is generally one-third of the whole, or = 2. So that, were we limited to such a depth of structure for both descriptions of girder, the open-work one should possess an advantage in point of economy of weight in the proportion of 5 : 6. When, however, the depth of the open-work girder is made one-eighth, and that of the plate one-sixteenth of the span, the ratios will be as 3 : 6; each part of the open-work girder, viz: top boom, bottom boom, and web, being then about half the weight of the corresponding part in the plate girder. This is a very rough comparison to make on such an important point, and may therefore be allowably objected to by the advocates of the plate form of web; it is, however, offered by the way, and we shall probably treat on the subject more fully at some other time. R.H.B.

EDINBURGH.

(To be Continued.)

Artificial Stone.

From the London Builder, No. 1008.

"Qute a new impulse" must now be given "to the external decoration of buildings," according to Dr. Frankland, F.R.S., in consequence of a discovery made and patented by Mr. Frederick Ransome, of Ipswich, in connexion with his well-known artificial stone, produced from water-glass and lime, with clay. Heretofore this artificial stone, after its formation in a plastic state, required about one month to dry, and another to be kiln-burnt. Now, it seems, the plasticity is superseded by stony hardness, not only without either drying or kiln-burning, but also in an hour or two's time! The moulded or prepared stone requires simply to be dipped into a solution, and the work is done, even when the stone has been one weighing a whole ton weight or more. The solution consists of chloride of calcium; or, as it is more properly called when dissolved in water, muriate of lime. When the

moulded matter is dipped into this solution it is very soon saturated with it, and a double decomposition takes place: the lime combines with the silica, forming a silicate of lime, which is one of the two chief cementing materials in all our cements, mortars, and concretes (carbonic acid being the other): the muriatic acid combines with the soda of the water-glass, forming common salt, which is said to be "washed off." How this can be, however, we do not see clearly: it must pervade the whole substance of the new-made stone; and might certainly be rapidly extracted by boiling water, or more slowly by cold water, or by rain *in situ*, leaving the stone more porous than before; but we do not mean to insinuate that hence the new-made stone cannot be a permanent and really a stony product. Indeed, Dr. Frankland's experiments with it seems to show that it must at once possess remarkably weather-resisting powers. On this point he says, keeping in view the result of his experiments, "I should scarcely have imagined it possible that an artificial material could in so short a time, and without any application of heat, have attained such water-resisting powers." It was found, however, as was to be expected, that the stone experimented on *contained* chloride of sodium; so that this salt had not been "washed off;" but it seems to be extractable without injury to the tenacity of the stone, although with an increase of porosity, as Dr. Frankland remarks. Of the general results of his experiments, Dr. Frankland says:—"Whilst they point out the Portland, Whitby, Hare Hill, and Park Springs, as the natural stones best adapted to withstand the influences of town atmospheres, they also indicate that Ransome's patent concrete will be found equal to the best of these in its power of resisting atmospheric degradation; and, if the newness of Ransome's stone (the specimen experimented upon not having been made a fortnight) be taken into consideration, together with the well-known fact that its binding material, silicate of lime, becomes harder and more crystalline by age, I am induced to hope that Mr. Ransome has invented a material which, with the exception of the primary rocks, is better capable of giving permanency to external architectural decorations than any stone hitherto used."

On reclaiming land from Seas and Estuaries. By Mr. J. H. MULLER (of the Hague).

From Newton's London Journal, June, 1862.

The author stated, that he understood works of this class to comprise an area either of salt marsh, samphire ground, slake, mud, or sand, lying more or less above the level of low water and being reclaimed from the sea by means of embankments, and drained by natural means through the sea banks. Reclaiming land was frequently looked upon as a hazardous speculation, owing to the probable contingencies where water had to be dealt with, and to the benefits being generally prospective. It was often condemned on account of the state of the ground, which was pronounced to be unsuitable for the pur-

pose. But the question should not be determined in that way, for the value of the ground before being reclaimed was no measure of the merit of the proposal, which could only be decided by comparing the cost of the necessary works with the improved value that would be given to the land when that operation had been accomplished.

After contending that the effect of reclaiming or draining land was to remove the cause of malaria or ague, and not, as had been erroneously asserted, to produce it, the author proceeded to point out, that in designing such works, the object should be to enclose the largest area with least length of bank, and the smallest average cross section. These points were regulated by the direction of the sea bank, to which attention was next called. It was sometimes recommended that the sea bank should be as nearly as possible parallel with the current, and at an angle to the prevailing winds; but experience seemed to show that, where creeks did not interfere, a different system was preferable; and that one side should be boldly exposed to the full force of the gales, and that the current should be allowed to act upon it almost at right angles, if, at the same time, that one side would shelter, or protect the two other sides. By this arrangement a less extent of bank required supervision during gales, and it also presented advantages during construction. The line of the embankment should, if practicable, cross creeks at right angles, and at the same level; and in all cases, care must be taken to secure the bottoms of the creeks by aprons, to prevent them from becoming deeper.

The extent of land to be reclaimed at any one time was then considered, and it was argued that large areas were the least expensive in the end; for if a small area was selected at first, some portion of the original sea-banks would be useless, when an increase became desirable. If the banks could not be constructed entirely on the salt marsh, it was preferable to go to half-tide level. The difficulty in the construction did not increase with the size of the area reclaimed, but depended upon the openings left in the banks. As instances:—In reclaiming a piece of land of 1000 acres, by a bank three-quarters of a mile in length, the seat of which was 6 feet below the level of high water, only one opening, 7 chains in width, was left. In another case, in reclaiming 1700 acres, by a bank four miles in length, the seat of which was 8 feet below high water, three openings, of 5, 7, and 12 chains in width, were left. In neither case was the speed of the outgoing current materially increased during the progress of the works, nor indeed until the cross section of the opening was diminished. In completing the latter work, the aprons were raised 18 inches, or 2 feet at a time, by wood work, stone, and clay. It was expected that the current would increase in the third opening, when the two others were raised; but this did not occur, as the water within the enclosure did not reach so high a level as that without; in fact, it never attained to high-water mark. When the aprons were above the level of the reclaimed land, the current on leaving became violent. This could not, however, be avoided, in finally closing a bank.

Between the old sea bank and the edge of low water, the soil might

be divided into four distinct classes,—the salt marsh, of clay, about the level of summer spring tides; then samphire ground, slake or mud, or rich alluvial matter, to half tide; next, hard sea sand; and lastly, near low-water mark, quicksand. Banks entirely on the salt marsh were the easiest and the strongest that could be made; those on samphire ground and mud were the most difficult. Slips were of constant occurrence, the use of wagons and horses was impossible, and a large proportion of the material was washed away as it was deposited, before the bank was consolidated, and raised above high-water mark. In fact, for waste, settling, and contingencies, from 60 to 100 per cent. of the original quantity must be calculated upon as necessary. If a storm arose, during the progress of the works, the slopes could not be protected; and indeed a bank constructed on such a bottom was always unsafe. When the line of the embankment was laid at the half-tide level, or about the limit of vegetation, and on hard sand, it was possible to make the whole of the reclaimed land fit for cultivation, and this plan need not cost more, and was safer, than by adopting the higher, but softer bottom. Banks on a lower level were not advisable.

Having stated the conditions to be observed in the direction and situation of the banks, the next question requiring attention was the cross section. This naturally divided itself into two parts,—the main body to resist the dead weight of the water when at rest, and the mode of protecting the slope, to enable it to resist the action of the water when in motion. With regard to the first point, the best cross section was that where the centre of gravity came nearest to the bottom, and to the toe of the bank. For this reason, steep slopes with a cess or bench, about the level of high water, were preferable to flat slopes without a cess or bench. Sand standing at its natural slope was sufficient to resist still water.

Breaches in banks were attributable either to a small percolation of water underneath the seat, or to the defence or protection of the slope being insufficient. Frequently it was not possible to obtain clay in sufficient quantities to form a puddle wall in the centre of the bank; and if the force of the wave was strong enough to break through stone and wood, clay would not be able to resist it. Sometimes, at extraordinary high tide, a breach would occur above the cess, but this rarely happened, and the time during which danger could arise was so short, that the evil might be remedied before the next returning high tide. When the water rose above the top of the bank, the back unprotected slope was liable to be damaged, and thus to lead to a breach. This might be averted by driving stakes into the top of the bank, and placing planks, supported by clay or other materials, behind them.

With respect to the protection of the slope, there was a difficulty in ascertaining correctly the force of sea-water when violently agitated. Mr. Storm Buysing had stated, in his work on hydraulic engineering, that the shock of the water and of floating objects against slopes, increased in the same ratio as the sine of the angle formed by the slope with the horizon. De la Coudraye and Brémontier contended, in

their theory of the motion of the waves, that the water only moved vertically up and down, without any horizontal displacement. It was well known, that the sea had the power to destroy banks, and to displace stones of considerable weight; and the engineer must be guided by experience, in dealing with these matters, rather than by speculative opinions.

The materials employed for the defence of slopes were of three different kinds,—clay and grass flags, wood, and stone. When banks were constructed on salt marshes, the body consisted of clay taken from the adjoining excavations. In this case it was advisable, after trimming the slopes, to sow coarse and meadow grass and clover seeds, and to protect the whole with a crammat. This crammat, which cost threepence or fourpence per square yard, was composed of a layer of clean barley straw, about 2 inches thick, evenly laid, and fastened to the clay by straw bands or strands; 60 to 90 stitches being made per superficial yard. In two or three years the bank was so consolidated, that the mat did not require renewal. When these banks were on a lower level than the salt marsh, a protection of clay and grass was insufficient. In such cases, a layer of clay, protected by stone, at a slope of 4 or 6 to 1, was employed in England, but without a cess or bench. This afforded the requisite strength, but it was expensive, and, as usually constructed, it needed much repair. The author thought that, when the bank was constructed on samphire ground, as within a comparatively short period a new salt marsh or fore-shore would be formed, it would be sufficient to protect the slope of the bank with wood, and that the slopes above the cess need not be protected, nor be flatter than 3 to 1.

A description was then given of the protection by fascine work. This consisted of layers of fagots, 5 or 6 inches in thickness, placed in a direction up and down the slope of the bank, the thick ends overlapping the thin ends of the lower rows. These were fastened down by stakes, which were left 8 inches above the fagots, and were connected together by means of willow binders, or "wattles," something like hurdle work. When the proper sort of wood was obtained, this protection would endure from five to seven years, and was quite able to resist the action of the tide. The strength of this kind of protection might be increased, by increasing the number of stakes and binders, or by filling in with stone, firmly wedged between the rows of stakes. The stone defence, as commonly constructed by the Dutch, on islands exposed to the ocean, was formed thus:—When the slope was trimmed, a layer of clay, 12 inches to 18 inches in thickness, was spread over it, covered sometimes with a crammat. Over this, bricks in one or two courses were laid, and then from 6 ins. to 12 ins. of brickbats, on which stones from 12 inches to 18 inches in depth were set. This work, though very durable, was costly, and hence should only be adopted where security rendered it necessary: as, for instance, for banks near to low-water mark. Details were then given of four different cross sections, and it was observed that, with a stone defence, the slopes were recommended to be flatter, and the banks to be high-

er, than where wood protection was employed; for it was expected that the former would be built in more exposed situations. In some cases it had been found advantageous to introduce rows of oak stakes, at intervals above the surface of the stone, to break the force of the waves.

In the construction of sea walls or banks, the most difficult operation was that connected with the crossing of creeks, before alluded to, especially when the bottom was 10 feet, 20 feet, or more under low-water mark. In England, the usual plan was to fill in large quantities of material from the sides; but this was a costly method. In Holland, on the contrary, the custom was to raise the bottom uniformly to the level of low water by means of cradles. The cradle was formed of brushwood, bound together by ropes and osiers, and was usually from 2 feet to 3 feet thick. It should be made on a flat sand, or silty ground, about 3 feet below high water, of the full length of the opening, and of proportionate width; being perfectly flexible, it adapted itself to the inequalities of the ground. It was stated, that particular attention must be paid to the stakes or fastenings by which it was held down, as the safety of the cradle depended entirely upon them. After being so secured, it was weighted with clay, brickbats, and stones. The mode of constructing a cradle, of floating it to its place, and of sinking it in the centre line of the intended embankment, were then minutely described. The sides of the opening were next protected with similar cradles, the lower end of each resting on that first laid. Subsequently, other cradles were sunk over these, until the work reached low-water mark, when the width of the embankment was gradually increased by throwing in sods on the flood side, protected by fascine work, weighted with stone. The same process was then pursued on the ebb side. When the surface of the creek was level with, or above, low water, cradles were not required. In such cases, the ground was covered with a thin layer of clay, protected by an apron of fascine work.

In conclusion, the mode of constructing the banks themselves, by side cuttings at least 20 feet from the foot of the slopes, was described; and it was urged, that each part undertaken should be raised to its full height in one tide, the exposed side being covered with a thin layer of clay. In the next tide, this should be provisionally protected by a crammat, and before the ensuing spring tide, the work should be finally protected with stone or wood.

Description of the Delta of the Danube, and of the Works recently executed at the Sulina Mouth. By C. A. Hartley, Assoc. Inst. C. E.

From the Civ. Eng. and Arch. Jour., April, 1862.

In the autumn of 1856, by virtue of the Treaty of Paris, the European Commission of the Danube, consisting of representatives from each of the seven contracting powers, was charged to execute the works necessary below Isakcha, to clear the mouths of the river as well as the adjacent parts of the sea, of the impediments which ob-

structed navigation. This Commission, to which the author had acted as chief engineer, was authorized to levy rates to cover the expense of such works, on the express condition that the flags of all nations should be on a footing of perfect equality.

In the preliminary studies of the three principal branches and mouths of the Danube, advantage was taken of the charts made by Captain Spratt, R. N., C. B.; and aided by these, and the author's own surveys and personal investigations, a brief description was given of the chief characteristics of the progress of the river through its delta. The Danube, after a course of 1700 miles, during which it received more than four hundred tributaries, and drained upwards of 300,000 square miles, passed in a single channel 1700 feet wide and 50 feet deep, the Bulgarian town of Isakcha, situated on the right bank, at 30 and 40 English miles respectively below the large corn-exporting ports of Galatz and Ibraila. Isakcha was 76, 78, and 90 miles from the sea, following the courses of the Kilia, the Sulina, and the St. George branches, and 58 miles in a straight line. The head of the delta was reached, at Ismail Chatal, or Fork, 15 miles lower down, and here the fresh waters divided, never to re-unite; seventeen twenty-sevenths of their volume passing in an easterly direction by the Kilia branch, and the remaining ten twenty-sevenths in a south-easterly direction by the Toultcha branch. At 11 miles below Ismail Chatal, this latter branch separated into two channels, the St. George and the Sulina, discharging respectively eight twenty-sevenths and two twenty-sevenths of the whole volume of the main river.

A short account was then given of the three channels, from which it appeared that the waters of the Kilia were delivered to the sea by twelve distinct mouths, only navigable for fishing vessels: that the river portion of the St. George offered no real obstacles, having an average width of 1200 feet, and a minimum depth of navigable channel of 16 feet, at seasons of extreme low water; and that in the upper reaches of the Sulina, disaster of every kind was imminent, from the many intricate windings and numerous shoals—the navigable width being rarely more than 300 feet, and the depth over the shallows during seasons of low water varying from 10 to 14 feet.

The delta proper was described as being bounded on the north by the Kilia branch, on the south by the Toultcha and St. George branches, and on the east by the Black Sea; the enclosed space comprising an area of 1000 square miles, and forming a triangle of which the Ismail Chatal was the western apex, and the sea coast, from the mouths of the St. George to those of the Kilia, the base. During extraordinary high floods, the delta, being unprovided with artificial banks to contain the swollen waters, was almost entirely submerged; whilst at seasons of drought, its banks were elevated from 10 to 12 feet above the level of the river at the Upper Chatal, and from 8 to 10 feet at the Chatal of St. George. In the lower reaches of the three branches, the level of the river was but little affected by variations in the upland waters. Adjacent to the mouths it never varied more than 1 foot, except when influenced by the wind. During high floods the

inclination of the surface water of the Sulina branch was 3 inches per mile, whilst during extreme low water it did not exceed 1 inch per mile. At times of ordinary high water, when the current had attained a velocity of from 2½ to 3 miles an hour, the Danube before it divided at Ismail Chatal delivered a volume of water equal to nineteen and a half millions cubic feet per minute; while in the dry season, when the current was reduced to 1 mile per hour, the flow did not exceed seven and a half millions cubic feet per minute. At times of extraordinary floods, such as that which occurred in March, 1861, the velocity was increased to 5 miles per hour, and the volume of water then delivered amounted to sixty millions cubic feet per minute, or eight times the quantity discharged at ordinary low water. It was stated, as the result of careful observations, that when the waters were most surcharged, they carried to sea at the rate of 1 cubic inch of sedementary matter, supposing it to be solidified into coherent earth, per cubic foot of water, and that not more than one-fortieth part of this proportion was transported when the floods had subsided. Thus, at the former period, upwards of 600,000 cubic yards of diluvial detritus passed into the sea by the several mouths of the river in twenty-four hours, and at the latter not more than 15,000 cubic yards. The results of these investigations accounted in a great degree, for the changes which took place from time to time, in the position and extent of the sand banks forming the bars across the several mouths. At times of high floods, these bars were further from the shore, their magnitude was considerably increased, and the depth over them was diminished; their distance from the shore, and their height, being much influenced by the direction of the prevailing winds. The depth of the sea opposite the delta decreased to the north; thus at three miles from the land, the depth was 16 fathoms opposite the St. George's mouth, and only 10 fathoms opposite the Sulina and Kilia mouths.

During the interval, from 1830 to 1857, the shallows of the Kilia advanced fully one mile in the direction of the Sulina mouth. This, combined with uncertain and changeable nature of the many branches issuing from the Wilkov basin to the sea, and the distance of the bars from the shore, were the chief considerations which induced the author to form an unfavorable opinion of the Kilia—in spite of its possessing the best river channel—and to recommend, in preference, the improvement either of the St. George or of the Sulina, where the sea depths were greater, and the advance of the sand banks was less remarkable. In comparing the merits of the two latter branches, the author arrived at the conclusion, that in nearly every respect the St. George offered decided advantages over the Sulina. It is true, that in order to reach the Kedrilles bar of the St. George, double the length of works would be necessary; but when once the sand-banks were passed, the greater sea depths opposite the St. George would insure for a longer period a constant good navigable depth at the sea entrance. The St. George's mouth was situated at the most saliant angle of the delta, was nearer to the Bosphorus, by 18 nautical miles,

than the Sulina, and was more favorably placed with regard to the safe manœuvring of vessels during N.N.E. winds.

Although there was a great difference of opinion as to the merits of each of the three principal branches, or mouths, all the technical authorities, who had studied the question on the ground, agreed in recommending that, whichever mouth is chosen, the system of improvement should be that of guiding the river water across the bar by means of piers projected from the most advanced dry angles of the mouth; so as to concentrate the strength of the river current on the bottom of the proposed improved channel, by an artificial prolongation of the river banks into deep water. After considerable discussion, the Commission resolved to improve the bar channel of the Sulina, by guiding piers of a temporary character, in order to give the speediest relief to the navigation in the cheapest manner; but it was distinctly guaranteed that this should not prejudice the choice of the mouth to be selected for permanent treatment. The author then received instructions to provide works which, for the expenditure of a sum limited to £80,000, should have the effect of giving an increased depth of at least two feet over a period of from six to eight years. This duration of time was based on the assumption that during such an interval, either the St. George would be opened, or it might be considered expedient to limit the improvement of the Danube to rendering permanent the provisional works.

The designs for the provisional works were then matured; and as it was found in practice, that the cost of strong timber cribs, to be loaded with stone, and sunk at intervals of 20 feet along the line of the works, would exceed the original estimate, choice was finally made of a structure composed of timber piling and pierre purdue, surmounted by a timber platform 14 feet wide, strengthened occasionally by solidly-constructed cribs of the same width. The works were commenced on the 21st April, 1858, a temporary staging fixed on piles, being always run out from 200 to 300 feet in advance of the permanent piling. The staging supported nine crab engines, by which three rows of three piles, each 13 inches square, and 7 feet apart, were frequently driven in one day to a depth of 16 feet into the hard fine sand of which the bottom was composed. The piles were immediately secured by double longitudinal walings and double cross-ties, the whole being surmounted by two thick tram-pieces and planking, at 4 feet above the level of the sea. From this permanent platform the close piling on the side next to the sea was driven. The daily rate of progress during fine weather, was 20 lineal feet; and as soon as this length of sheet piles was completed, stones were thrown down to protect the footing in the sand, which was liable to be washed away by the action of the sea. This scouring action of the sea was so serious, when the skirt of the bar was reached, that it threatened at one time to demand, for the completion of the works, double the quantity of stone originally estimated. Several plans were tried to reduce its pernicious effects. That eventually adopted, and which was perfectly successful, was to advance the open pile work with all possible expedi-

tion, and then to pave the proposed seat of the pier with stones delivered from barges. This pavement withstood the attacks of the sea, and offered no great obstruction to the penetrations of the sheet piles, which, without being shod, had frequently been driven 10 feet into the ground, after having been forced through 8 feet of rubble stone. The section of the finished stone work was described as being a solid mass of closely packed third class rubble, resting on a broad base, and narrowing upwards at slopes varying from 2 to 1, near the pier heads, to 1 to 1 and 1½ to 1 near the shore, until slightly below the level of the water it became a mere ridge against the close piling. The time occupied in the actual construction of the pier was thirty-one months, exclusive of three winter months in each year, during which the Danube was frozen over, and all work was suspended, but inclusive of two hundred and seven days, when it was impossible to work on account of stormy weather. The length of the north pier was 4631 feet, that of the south pier was 3000 feet, and the depth of water in which they were built varied from 6 to 20 feet. In their construction, 200,000 tons of stone and 12,500 piles had been employed, and the cost had not exceeded ten guineas per lineal foot. The stone was brought from a distance of 60 miles, and its price delivered in place varied from four shillings to five shillings per ton; the oak used for the longitudinal and transverse timbers and for the planking and fender piles, cost two shillings and threepence per cubic foot, while the fir timber piles were delivered ready for driving for fourpence per cubic foot. The workmen, of whom there were generally 300, were composed of men belonging to more than ten different nations. Laborers were paid two shillings and sixpence and carpenters four shillings and sixpence per day.

The changes which had taken place at the Sulina mouth, consequent on the projection of the piers, were then noticed. The depth on the bar since the year 1829 had varied between the extremes of 7 and 12 feet, the least depth occurring during the subsidence of high-water floods, and the greatest when the deposits lodged by those floods had been dispersed by autumnal and winter gales. In April, 1858, when the works were commenced, there was a navigable channel only 9 feet deep over the centre of the long shoal forming the Sulina bar. In November, 1859, when the works had been brought to a close for the winter, the north pier had advanced 3000 feet, and the south pier 500 feet, and then the depth on the bar was 10 feet, which was increased to 14 feet by the following April, although the works had remained stationary. Hopes were consequently entertained that the action of the north pier would, in itself, be sufficient to maintain an improvement; but these expectations were disappointed, as in August, when the north pier had reached a length of 4600 feet, the depth on the bar had diminished to 9¼ feet. Every exertion was then made to bring the opposite pier into play. Accordingly, during the next three months, the south pier was advanced 1500 feet, and as it was now within 600 feet of the north pier, the good effect of concentrating the whole force of the river current directly on the bar became at once

apparent. Thus, on the 30th of November, 1860, there was a navigable channel of 12 feet, and on the 28th February, 1861, of 16 feet. Then came the breaking up of the ice in the river, and the furious descent of the extraordinary high floods, which caused so much damage at Galatz, and submerged the whole delta; but this time instead of the depth on the bar being diminished, the swollen waters confined between the two piers and directed in a proper line, fairly swept away the remains of the bar on to the south bank and into deep water.—From that time to the present, the depth had never been less than 16½ feet, and frequently it was as much as 17½ feet over a navigable width of 500 feet. This result had been accomplished by works the cost of which had not exceeded the sum that had been paid in one year only for lightening vessels over the bar; and without taking into account the excellent shelter which had been afforded, and the great risks which vessels formerly ran of being wrecked off the entrance.

In conclusion, the author expressed his gratitude to the members of the European Commission of the Danube, for the generous support he had always received; and especially to Major Stokes, R. E., the representative of Great Britain, whose enlightened policy, if allowed to prevail, could not fail eventually to insure to the commerce of all nations, the best possible means of water communication with the rich corn-growing countries bordering the shores of the Lower Danube.

On the Electrical Tests employed during the construction of the Malta and Alexandria Telegraph, and on insulating and protecting submarine cables. By Mr. C. W. Siemens, M. Inst. C. E.

From Newton's London Journal, July, 1862.

Having been employed by Her Majesty's government as the electrician to superintend the manufacture and shipment of the Malta and Alexandria telegraph cable, the author was in a position to speak as to its actual state of insulation, at different stages of its progress, and as to its general superiority compared with former lines. The methods of testing differed essentially from those previously resorted to. This was the first line that had been tested systematically throughout; and the importance of a uniform and well-devised system of electrical tests being carried on during the manufacture, shipment, laying, and subsequent working of submarine cables, had been fully proved.

The covered strand of conducting wire, in lengths of one nautical mile, was placed for twenty-four hours in tanks filled with water maintained at 75° Fahrenheit. It was afterwards removed into a pressure tank, containing water at the same temperature, and when uniformly heated, it was tested for conductivity and insulation, and the result, expressed in units of resistance, noted. A pressure of 600 pounds per square inch was then applied, and the electrical tests were repeated. Before any coil was approved, it was required that the copper resistance should not exceed 3·5 (Siemens) units, or possess 80 per cent. of the conductivity of chemically pure copper; that the gutta-percha resistance per knot at 75° should amount, at least, to ninety million

units, corresponding to 80 per cent. of the highest insulation that could be obtained with the best gutta-percha of commerce; and further, that the insulation should improve when the pressure was applied, which was invariably the case when the covering was sound. The coils were then transferred to Messrs. Glass, Elliot & Co.'s works at Greenwich, where they were submerged in tanks until required for the sheathing machine. The sheathed cable was coiled into large tanks, and was always intended to be covered with water, but owing to a defect in the construction of the tanks, this regulation could only be partially carried into effect. It was also intended, in the first instance, that the ships should be provided with water-tight tanks to receive the cable during the outward voyage; but owing to the passive resistance with which every deviation from previous routine was usually met, this plan was not carried out, until the heating of the cable on board the S.S. *Queen Victoria* had proved, at great cost, that tanks were essentially necessary. There were other important advantages obtained through the adoption of the water tanks, by which the causes of failure in paying-out were avoided, and the operation was rendered comparatively safe and easy.

In conducting the electrical tests of the Malta and Alexandria cable in the course of its manufacture, the chief object was to obtain throughout strictly comparative results. For this purpose it was necessary to adopt a standard measure of resistance, by which to express both the conductivity of the copper conductor and of the insulating covering. The standard measure had been supplied by Doctor Werner Siemens. The unit of resistance was that of a column of pure mercury, contained in a glass tube, one metre in length between the contact cups, and of one square millimetre sectional area, taken at the temperature of melting ice. As the testing apparatus had been already described in the Blue Book "On the Construction of Submarine Cables," it was not necesary to repeat it. In the appendix to this paper, tables were given of the results of observations upon two sections of the cable, at various stages of their progress, between Malta and Tripoli, and between Tripoli and Benghazi; and diagrams were exhibited representing graphically these results. On comparing the insulation of the cables after being laid down, with the insulation observed shortly before on board ship, there was a decided improvement after submersion. This was partly due to the pressure upon the cables, the insulation improving 2 per cent. on an average for every 100 pounds of pressure upon the square inch, and partly to the lower temperature at the bottom of the sea.

For working the line, Messrs Siemens, Halske & Co. had supplied ink-recording instruments, fitted with peculiar arrangements for discharging the residuary charge of the cable, and capable of being worked by exceedingly feeble battery power. Although the line was divided into three electrical circuits, messages were transmitted mechanically and instantaneously, at the intermediate station, by a system of double relay, or translation. By this plan messages could be sent instantaneously from London to Omsk, in Siberia, and there would be no

electrical difficulty in establishing the same direct intercommunication between London and Calcutta.

Respecting the construction of a cable of a more permanent character than any hitherto made, to which the author had given much consideration for many years, it was observed that, with regard to the insulating covering, nature seemed to have provided only two suitable substances—india-rubber and gutta-percha, combining permanent pliability at all ordinary temperatures with high insulating property.—India-rubber had a higher insulating power, a lower specific induction, and was capable of resisting higher temperatures than gutta-percha; but the latter could be put upon the wire in a plastic state by a die process, and gave greater security against faults than the lapped india-rubber covering. It was also less liable to receive accidental injuries, to become sticky or semi-fluid when exposed to the atmosphere, and resisted the action of water more perfectly.

The absorption of water by gutta-percha, india-rubber, and compounds of india-rubber, such as vulcanized india-rubber, Wray's mixture, and a compound with mica, under various pressures and temperatures, and from water containing different degrees of salt in solution, had been fully investigated. These experiments served to show, that an increase of pressure up to 50 pounds per square inch, did not increase the rate of absorption, which was found to be more rapid from pure water than from sea water, and from sea water than from brine. Raw and unvulcanized india-rubber absorbed water in greater quantities than the other materials; while next to gutta-percha, vulcanized india-rubber showed, both in fresh and salt water, the greatest insensibility to absorption.

The results of experiments on the insulating and inductive capacities of wires coated with india-rubber, in combination with gutta-percha, compared with those of special gutta-percha and pure india-rubber at different temperatures, were then given. The lengths experimented upon varied from 600 to 2500 yards. The specific resistance of special gutta-percha decreased from 9·11 at 50° Fahrenheit, to 1·50 at 80° Fahrenheit, or to about one-sixth of its original value; while the combination of india-rubber and gutta-percha had, under the same circumstances, only gone down to about one-third of its insulation at 50° Fahrenheit. The inductive capacity of the combined india-rubber and gutta-percha wire, and of pure india-rubber covered wire, was as 0·7 to 1. Notwithstanding the comparatively high insulating property of india-rubber, its low inductive capacity, and its power to resist heat, its gradual dissolution in sea water was a circumstance which alone rendered it inadmissible for submarine wires, unless it was securely enclosed in another water-proof medium, and gutta-percha appeared, in every respect, well suited for such outer covering. It was desirable that the india-rubber should be brought upon the wire without the application of heat or solvents, both of which often entailed a gradual decomposition of that material, particularly when exposed to atmospheric influence in contact with copper. Dr. W. A. Miller had stated that the liquefaction was the result of a

process ef oxidation, from which it might be inferred, that the effect could not take place where oxygen was excluded. It, moreover, was important to produce a perfectly cylindrical covering, and taking advantage of a peculiar property of india-rubber cohering perfectly where two fresh cut surfaces were brought together under considerable pressure, the author had constructed a covering machine which fulfilled the several purposes. Such combined india-rubber and gutta-percha covered wires had been tried under various circumstances, exposed to the atmosphere, to water, or the moisture of the ground, for nearly two years, without betraying any signs of gradual deterioration of the india-rubber, or the appearance of faults. A circumstance greatly in favor of the bi-covered wire was, that the gutta-percha shrank upon the india-rubber covered wire, and when any mechanical injury to the covering occurred, the yielding india-rubber was forced into the gap by the elastic pressure exercised by the gutta-percha, and prevented the appearance of a fault.

The outer covering of cables, as heretofore constructed, was certainly the least perfect part. An iron sheathing was very necessary to protect the insulated core in shallow waters, but for cables in more than 30 or 40 fathoms of water, the iron sheathing was an element rather of weakness than of strength; it rendered the cable ponderous, its shipment expensive, the paying-out risky, and repairs impossible; owing to the difficulty of raising a heavy cable from a great depth under any circumstances, and the absolute impossibility of doing so after corrosion of the iron wire had made some progress.

When the Falmouth and Gibraltar cable was first contemplated, the author, in conjunction with Mr. Forde, proposed to cover each iron wire with gutta-percha, with a view to prevent oxidation; but the system was not acted upon, except by way of experiment. Mere protection of the wire was, however, not sufficient, in the author's opinion. It was capable of mathematical demonstration, that in paying-out a wire-sheathed cable, with a considerable strain upon the brake-wheel, it would untwist while in suspension in the water, to a considerable extent, causing elongation of the core to the amount of say one per cent., or even more. On reaching the bottom, the strain and consequent twist would be released. Copper wire could not be elongated more than 2 per cent. without receiving a permanent set; and it was also a well-ascertained fact, that when a telegraph core had been stretched at any time beyond the limits of elasticity of the copper, the latter, being henceforth too long for the more elastic covering, would tend to assume a serpentine form, and to push its way through the insulating material by slow degrees, particularly in places where short bends or kinks occurred.

Based upon these views, the author designed a sheathing of the following description:—The insulated conductor or core was passed in the sheathing works through a series of three machines in close succession. In passing through the hollow spindle of the first machine, a close spiral covering of hemp, previously saturated in Stockholm tar, was applied in such a way, that each string was and remained under

a given strain. The second machine was similar in construction to the first, but supplied a second covering of hemp wound in the opposite direction to the first. The rope, thus formed, passed next through a stationary clip, with longitudinal grooves, to prevent it from turning round in the operation immediately following, which consisted in the application, under the influence of great pressure, of from three to six strips of copper or other metal, which might best resist the action of sea water. These strips were accurately guided into the revolving covering tool, so as to overlap each other equally for nearly half their breadth; the pressure applied being sufficient to crush or socket the one metal down where it was covered by the other. This cable had no tendency to untwist; its extension, with half the breaking strain upon it, did not exceed one-half per cent., and being very strong, and of only double the weight of water, it would support about eight miles of its own weight in the sea.

Considering that good ships sheathing lasted about ten years, when the ship was at rest, and that the cable had two layers of metal, with hardened tar between, it appeared not unreasonable to suppose, that this sheathing would last at the tranquil bottom of the ocean, from twenty to thirty years at least. Several short lengths of this cable were now being tried, under various circumstances, and the results, so far, were promising of success upon a larger scale.

MECHANICS, PHYSICS, AND CHEMISTRY.

International Exhibition.—The American Court.

From the Mechanics' Magazine, June, 1862.

We resume our notice of this portion of the Exhibition in a desultory kind of way. Indeed, no other course is permitted to us, as the arrangement of the Court is itself desultory. No kind of classification has been made, and we believe at one time it was doubtful whether an American Court would be opened at all. Even now we may say that the arts and manufactures of the United States are unrepresented. The collection we have in the south-eastern angle of the building is not here through the patronage or encouragement of the United States government, which has other work on hand. It was through the enterprise and creditable ambition of a few private individuals, who were determined to show, at their own risk and on their own responsibility, that there is inventive skill in America, which is not afraid to measure itself against that of Europe in the same direction. Not a single Southern State is represented. This may be on account of the blockade, but it is said that had there been no blockade at all, the South, though she might have shown well in natural productions, could have made no figure in manufactures or mechanical inventions. Only two or three patents have been taken out in the Southern States for many years, and these in the Carolinas. The State of New York, and the Northern States generally, have been

the fertile source of all the inventions which have been patented since the Exhibition of 1851.

The United States Court is represented by Mr. Holmes, and his son-in-law, Mr. Taylor, an eminent engineer. It is due to the energies and sacrifices of these gentlemen that America is represented at all, and the things which their public spirit has brought together, indicate the actively practical mind of their countrymen. There are nearly a hundred articles in the various classes exhibited, and most of them have the merit of novelty as well as utility.

As one might expect, sewing machines are well represented—those of the lock-stitch, the chain-stitch, and the shuttle machine, with all their peculiarities. They are successfully exhibited, and form a great attraction. The rapidity and neatness with which these machines execute a variety of needlework, is amazing to those who know only of the common needle as the grand making and mending instrument in the household, and the symbol of the most distressing drudgery. As we intend to devote a special paper to these machines, we pass them by at present. In class 1, Mr. Feuchtwanger exhibits a thousand specimens of minerals; Mr. Meads, from Lake Superior, and the New Jersey Zinc Co., specimens of zinc ores with their products, pig and bar iron, and steel. In class No. 2, Mr. F. S. Pease, of Buffalo, has a variety of mineral and animal oils for use in lubricating machinery, and as illuminating agents.

The various oils are shown to great advantage in glass cylinders of various altitudes, and appear to attract great attention. We have coal oil for lubrication; oil from tar for machinery; also signal oil, that is, oil which may be used on locomotives, on the foremast of a ship, or on a railway signal; we have oil so limpid that it adapts itself excellently to the rapid motions of the sewing machinery, as it never gets gummy. There is a sample of oil from compressed lard of amazing transparency. The latter goes by the name of winter oil, as at 3° under freezing point, it never coagulates, and is admirably adapted for the lamp in cold climates on that account. The engine and machinery oil is equal to sperm, and much cheaper; it stands a greater degree of heat and a greater degree of cold than sperm oil, and does not consume so fast. Mr. Pease has samples of petroleum in the crude and refined state, which cannot be exhibited in the building on account of the fire insurance policy. There is, further, an oil shown called "armor oil," which is intended especially for gun-locks, and in which our volunteers may perhaps feel an interest on account of their Enfields and Whitworths.

By the side of these oils are exhibited hops, seeds, wheat, beans, peas, buckwheat, and samples of starch and flour manufactured from Indian corn or maize, of which there are shown a number of specimens in the ear. The starch is extolled for the gloss it gives to the linen or cotton to which it is applied. The flour is remarkably white and fine. Samples are here, too, of a farinaceous article manufactured by the Glencove Starch Company of New York, under the name of "Maizena." It is the purest preparation of the finest maize.

In a short time, and without any trouble, it can be made into various forms of diet, and is a good substitute for arrowroot.

Having disposed of these odds and ends, we proceed to notice a few mechanical contrivances, and, first, near the south-east entrance, one is attracted by a cork-cutting machine invented and patented by Mr. Conroy, of Boston. There is one machine which cuts the cork into parallelopipedons, and then into smaller figures of the same kind, according to the length of bung or cork required. These smaller pieces are brought in contact with a knife mounted on a circular horizontal disk. The disk is put in motion by a large wheel similar to a cutler's wheel, and a band running over a drum in immediate connexion with it; or it may be worked by steam power. This disk, by means of gearing, traverses a platform from right to left, and *vice versa*, by which arrangement, a cork is no sooner cut on one side than a cork is cut on the other. The square body to be transformed into a rounded is placed in a groove; the gearing seizes it in the manner of a piece of wood in a turning lathe, by its extremities, advances it to the edge of the circular knife, and in an instant the rough block of cork appears a shaped article wherewith to stop a beer barrel, a bottle of champagne, or a medicine vial. The ease with which this machine does its work is surprising. A clever cork-cutter, working by the hand, can turn out, on the average, eight gross of corks a day. By this machine can be made fourteen gross of corks per hour. In a day of ten hours, therefore, two men can produce 20,160 corks or bungs, while two men by the hand, in the course of the same time, can turn out only 2304. The corks can be cut in perfect cylinders, or beveled to any angle required by slightly elevating the horizontal disk. The machinery is very simple, and ingenious through its simplicity.

A bolt is shown in one part of the Court, which has all the excellency of the rivet, with this advantage over a rivet, that when required it may be moved from its place without any trouble. It is well adapted for the frame-work of locomotives and railway carriages. The bolt passes through an iron frame, or through wood-work, and is secured behind by a nut. But inasmuch as a nut is liable to be unturned in the extremity of the thread of the screw-bolt by vibration, and as many railway accidents have happened from the fact of bolts having parted for the want of their retaining nuts, in the present case the nut is kept into its place by having a spring inserted into it, which adapts itself to the ratchet-work of a hollow washer. The inventors are Messrs. Lawrence and White, of Melrose, N. Y. Close to the screw-rivet bolt, is a contrivance for common land carriages. A coupling iron, which accommodates itself to the oscillations of a carriage on a rough road, without inconvenience to the horse or horses, and which, fitted on the fore axle of a four-wheeled vehicle, answers all its radical motions, without being pinned like the bolt under the axle. It is, in fact, a kind of universal joint, answering to every motion of the carriage or of the horse, and which, if adopted generally, is likely to prevent many accidents.

Scholl's life-boat is constructed on a novel principle. The model

exhibited is rather a rough one. It looks like a great green porpoise, with a lid opening into his back. Look into the interior, however, through the lid, and you discover the arrangements for the accommodation of a crew and passengers—for the saved and the saviours, as the case may be. The object of the boat is to pass through a heavy surf with safety. The internal fittings of the boat are below the centre of gravity and of flotation. They are hung in the manner of a binnacle compass, that is, be the motion of the external shell or hull of the boat what it may, the persons within are always maintained in a horizontal position. Indeed, let the boat turn round and round like a spindle, which is hardly possible, its passengers are nevertheless unmoved. The steering apparatus is within, and so also all the arrangements for a screw propeller. This boat has no outer deck; indeed, as we have said, in form it resembles a porpoise in the model, and on a large scale it must be something "very like a whale."

There are four exhibitors of pianos, all of New York city or county. These instruments vie in tone, and power, and in cabinet work, with any in the other courts of the building. In power, we suspect that they will carry off the prize against all competitors. We had the opportunity, at least, of listening to a square and a grand exhibited by Steinway and Sons. The internal arrangements of these instruments are novel; the strings are not all in parallels like those in the usual pianos; on the contrary, the bass strings are placed at acute angles above the tenor and treble strings, and obtain the full advantage of the sounding board. The motions of the hammers are not impeded by this arrangement. The grand has seven octaves, and tone loud enough for a large concert room, and yet, through the mechanical arrangements of the instrument, it can be made to play as softly as if it had been intended for a sick chamber. Amidst the many musical instruments to be found throughout the building, the visitor, curious in these things, should by all means see the pianos in the American court.

On the Law of Expansion of Superheated Steam. By Wm. Fairbairn, Esq., LL. D., F. R. S., and Thomas Tate, Esq. Received March 20, 1862.

(Abstract.)

[From the Proceedings of the Royal Society, No. 49.]

In a former paper selected for the Bakerian Lecture, entitled "Experimental Researches to determine the Density of Steam at different Temperatures, and to ascertain the Law of Expansion of Superheated Steam" (Phil. Trans. 1860, page 185*), it was shown that although Dumas, Gay-Lussac, and other distinguished physicists, had determined the density of steam at 212°, it was, however, left for these researches to ascertain the law of density, volume, &c., at all temperatures, and also the law of expansion of superheated steam. These experiments have therefore been continued, and have elicited re-

* Journ. Frank. Inst., vol. xli., page 379.

markable results as regards the rate of expansion at various temperatures.

The earliest experiments on this subject were made by Mr. Frost in America, but without sufficient accuracy to be of scientific value. Mr. Siemens has also experimented on steam isolated from water; his results give a much higher rate of expansion for steam than for ordinary gases; but, owing to some obvious defects of Mr. Siemens's method of conducting the experiments, we consider his results are not reliable.

For gases, the rate of expansion is expressed by the formula for constant volume,

$$\frac{P}{P_1} = \frac{E + t}{E + t_1}, \quad \ldots \ldots \ldots \quad (1)$$

where E is a constant determined by experiment, and decided by Regnault as 459 in the case of air. In the paper alluded to, it was shown that, with a certain proviso, the rate of expansion of superheated steam nearly coincided with that of air. Within a short distance of the maximum temperature of saturation, the rate of expansion of steam was found to be exceedingly variable; near the saturation-point, it is higher than that of air, and decreases as the temperature is increased, until it becomes sensibly identical with that of air. The results upon which this law was based, were too limited in their range for much numerical accuracy in the constants deduced.

Hence, it has been our object in the present paper to supply the deficiency in the previous one, by affording experimental data of the expansion of steam at higher temperatures, and with a greater range of superheating than was possible with the apparatus employed in ascertaining the density of steam. The results obtained in these later experiments, however, confirm the general law deduced from the previous ones.

The apparatus used when the pressure did not exceed that of the atmosphere, consisted of a glass globe 3 inches in diameter, and stem 35 inches long; the capacity was known to a point where a piece of platinum wire was twisted to mark accurately the point at which the mercury column in the stem was to be brought to maintain a constant volume in the globe.

A $1\frac{1}{4}$-inch tube, filled with mercury, rested upon the frame, and enclosed the lower open end of the stem of the glass globe. The weight of the tube and frame was counterbalanced by weights. By such an adjustment, the tube could be regulated with facility, preserving the upper level of the mercury column at one uniform height. A cathetometer with vernier, to read the lower and variable level of the mercury column, was introduced. To heat the globe, the oil-bath was used, fitted to the tube by a stuffing-box; the oil-bath is itself immersed in a mercury bath, surrounded by a coil of jets of gas.

The globe, filled with dry and warm mercury, the air-bubbles being extracted by means of an air-pump, was inverted to form a Torricellian vacuum. A small glass globule of water was then inserted, the

platinum wire fixed in its place, and an india-rubber cap fitted to the extremity of the stem. Being transferred to its place, and the india-rubber cap replaced by an open glass cistern, so that the glass could be elevated to its position, the jets were lighted, and the temperature elevated to 300°.

From this point, the levels of the column were read off at intervals of 50°, until the temperature of saturation was reached. The levels were taken in a series of descending temperatures, to avoid the influence of steam boiling out of the mercury as the temperature rose, and to eliminate the effect of the cohesion of the glass on the water, as explained in our previous paper on the density of steam.

Twelve cubic inches of mercury were measured into the globe, and a file-mark made on the stem, below which, at a distance of 14·45 inches, another file-mark was made, affording a fixed point for ascertaining the correspondence of the upper file-mark with the readings on the cathetometer.

Let a be the reading on the fixed rod of the level of the column, b, the reading of the lower file-mark on the globe-stem; then $b - a$ = the height of the column of mercury on the globe-stem.

To correct for temperature, 7½ inches of mercury, enclosed by the oil-bath and its stuffing-box, were corrected for the temperature of the oil, and the remainder of the column for the temperature of the atmosphere at the time. By deducting the column so corrected from the reading of the barometer at the time, the total pressure in the globe is obtained. The readings of the thermometer are corrected for the portion out of the oil-bath. The pressure of mercurial vapor is calculated from data supplied with great courtesy by M. Regnault, and embodying the results of unpublished experiments. The pressure of this vapor is assumed to be the same as that in a vacuum, as the vapor in the globe remains still for a sufficient time (it is believed) for saturation to take place. In this view we have been strengthened by M. Regnault's opinion. By deducting the pressure of mercury vapor from the total pressure in the globe, the pressure of the steam is obtained.

On referring to the experiments contained in the paper, it will be seen that the law of expansion of gaseous bodies is expressed by the formula,

$$\frac{E+t}{E+t_1} = \frac{P\,V}{P\,V_1}; \qquad \therefore \qquad E = \frac{P\,V\,t_1 - P_1\,V_1\,t}{P_1\,V_1 - P\,V},$$

where E is a constant. Taking Regnault's constant 459 as the rate of expansion of air for constant volumes, a remarkable coincidence will be observed in the experiments contained in the paper when reduced to the same standard of value. The values of E thus deduced have been placed in the last column of the calculated experiments. They show a decreasing rate of expansion from the saturation-point upwards, until, at no great increase of temperature the rate of expansion coincides with that of a perfect gas.

Taking from the tables the two results, which in each instance re-

present the case of expansion at the greatest distance from the saturation-point, we have the following values of E:—

E = (1)	474·48	(3)	466·85	(5)	460·28
	450·11		451·94		
(2)	455·57	(4)	464·83		
	443·86		460·49		

Mean value of E deduced from these numbers = 458·74.

Hence, the conclusion which we suggested in our previous paper has been satisfactorily demonstrated by a more extended series of experiments, and the rate of expansion of superheated steam is shown to be almost identical with that of air and other permanent gases, if calculated at temperatures not too close to the maximum temperature of saturation.

New Gas Engine.

From the Lond. Civ. Eng. and Arch. Journal, July, 1862.

It is stated that M. Lenoir and MM. Mays Brothers have succeeded in constructing engines from ½-horse to 3 or 4-horse power, actuated by means of the expansion of ordinary coal gas, inflamed by electricity so as to act by its dilatation on each side of the piston alternately, without any steam. As this is not an experiment, but actually in practice with economy, the idea is neither "speculative" nor chimerical; so it deserves notice. A M. Révillon says, "I had four men to turn grindstones; they cost me 12 francs per day; I placed in their stead one of these auxiliary engines of one-horse power, which does all the work of the four men willingly and noiselessly for 6 or 7 francs a day."

Another, M. Bourgerie, states the working of a larger engine than the latter. "I wanted," he says, "a motive power, and I was not authorized to put up a steam engine; I have placed in my factory an engine (of the above nature) of 3-horse power, which has relieved me from the *embarras* in which I was placed. A machine like this can render the same service, in a thousand similar circumstances, to the industry of our populous quarters."

The following is a description of the engine; it consists of a horizontal cylinder on a bed of cast iron, as in ordinary steam engines, the connecting rod driving a crank upon the shafting, with fly-wheel and the usual gearing for distributing the motive power. By the usual eccentric and slide valve the gas is admitted above and below the piston. Two of Bunsen's batteries are put into communication with Ruhmkorff's induction coil, whence the electric current is conducted by two isolated wires to a distributor of electricity placed in front of the cylinder, so that the movement of the piston regulates the communication of the wires to the "inflamers" (*inflammateurs*) placed inside each disk of the cylinder, so that an alternate illumination takes place above and below the piston, and produces the required horizontal movements. The conserved gases are driven off by a waste pipe from the valve box. The whole is kept cool by a stream of water, the cylinder in a jacket.

To set it in motion, all that is necessary is to turn on the gas and give a help to the fly-wheel. As M. Leon Foucault says, it can be set in motion or stopped "*avec la même facilité qu'on allume ou qu'on souffle une bougie.*"

Steam Boiler Explosions.

From the Journal of the Society of Arts, No. 499.

At the last ordinary monthly meeting of the Executive Committee of the Association for the prevention of Steam Boiler Explosions, Manchester, held May 27th, 1862, Mr. L. E. FLETCHER, Chief Engineer, presented his monthly report, of which the following is an abstract:—

During the last month there have been examined 316 engines and 464 boilers. Of the latter, 9 have been examined specially, 10 internally, 74 thoroughly, and 371 externally; in which the following defects have been found:—Fracture, 6 (3 dangerous); corrosion, 50 (7 dangerous); safety-valves out of order, 6; water gauges, ditto, 15; pressure-gauges, ditto, 5; feed apparatus, 1 (dangerous); blow-off cocks, ditto, 34 (2 dangerous); fusible plugs, ditto, 6; furnaces out of shape, 6—total, 129 (13 dangerous). Boilers without glass water-gauges, 29; without pressure-gauges, 10; without blow-off cocks, 34; without back pressure-valves, 33.

Explosions continue to occur to every description of boiler. One has happened during the last month to a locomotive in the vicinity of London, and another to a stationary boiler on the Clyde. Both of these were—as it is hardly necessary to state—beyond the sphere of the operations of the Association. Each of these explosions was attended with fatal consequences, but I am not at present in possession of the engineering facts.

The recent very frequent occurrence of explosions has monopolized, by the bare recital of their details, the entire space of the late reports, and left no room for considerations resulting from them, to which I think it is important attention should be drawn, and this, therefore, I now propose to do.

It will be remembered that one of the late explosions arose from the failure of an angle iron, on which alone—as on a single thread—a large crown plate depended for its support. Several other explosions occurred to externally-fired boilers through failure of the plates just at the seams of rivets exposed to the flame. In some of such cases the plate is found to crack at the rivet holes; in others leakage occurs, from which corrosive action sets in, and steadily continues until the plate becomes so thinned that rupture and explosion ensue. Some explosions have occurred from corrosion, consequent upon external damp; others from acidity of the water; while others again, of somewhat earlier date, have been occasioned by the collapse of the furnace tubes, consequent upon the weakness of construction, which would have been remedied by the adoption either of flanched seams, T or angle-iron hoops, or other similar means.

Thus it will be seen that all the above explosions occurred from the

most simple causes, and that no mystery whatever need be attached to any one of them; while by suitable construction of the boilers in the first place, and due attention to their state of repair in the second, these explosions would in every case be prevented.

I am extremely desirous to keep this practical view of steam boiler explosions constantly in sight, since I am persuaded no head will be made against them generally so long as their causes are considered to be matters of mystery, and their occurrence one of chance.

Very few of the explosions that come under my notice occur from shortness of water, and I believe that to be a much-abused idea, and the number of explosions resulting from it to be much exaggerated. It appears to be an almost stereotyped verdict at inquests, and the boiler attendant being frequently killed, there is seldom any witness to the contrary.

I find that by far the most frequent cause of explosion is the insufficiency of the boiler for its working pressure, either on account of its original construction, or state of repair consequent upon use; while those explosions resulting either from deficiency of water, or from extraordinary or reckless pressure, are comparatively rare. In other words, to prevent misapprehension, I find that explosion is more frequently due to weakness of the boiler than to excessive pressure of the steam.

I know no means of ascertaining the sufficiency of the original construction of a boiler, or of testing the weakness produced upon it by wear and tear—in short, of testing either new or old boilers—equal to the use of hydraulic pressure, and think all steam users would do well to make systematic use of this test once a year. In France, I believe, this plan is rendered compulsory by the government, and it would be well were it generally adopted in this country voluntarily. Weak places in the plates may pass undetected, even on careful examination, while some parts may be inaccessible and concealed from view, but the hydraulic test is sure to detect and expose them all. Its timely application would have saved that most disastrous explosion which occurred some time since, here in Manchester, at a locomotive establishment second to none in the kingdom for its high reputation, and since a defect passed unnoticed at such an establishment, where the construction of boilers, as well as the quality and strength of the plates, may well be supposed to have been thoroughly understood, it surely argues the necessity of the hydraulic test being applied.

Mr. Muntz, a steam user in Birmingham, states, in a letter published on the Millfield boiler explosion, that he has for years adopted, with advantage, the plan of an annual hydraulic boiler test, and considers it a duty he owes to his workmen in consideration of their safety.

The application of the hydraulic test is so simple, and the pump required so small, that each steam user could provide himself with one at very little expense, or some parties might find it worth their while to take up the proving of boilers by water pressure as an itinerant speciality of engineering practice. This Association would be

glad to assist in the general application of the hydraulic test, by inspecting the boilers when under pressure, and I feel convinced that, were the practice of this annual test generally adopted, which I trust it soon will be, explosions would become nearly, if not entirely, extinct.

New Method of Amalgamating Zinc.

M. C. Delamarre in a communication to the *Cosmos*, states that he has found very satisfactory results in his galvanic batteries by dissolving from 12 to 25 grammes of bi-sulphate of mercury in the acid of each cell of a large-sized Bunsen battery. He states that with this precaution the zincs are always well-amalgamated, dissolve very slowly, and never show the least sign of effervescence.

The Ordinary Sextant.

From Newton's London Journal, July, 1862.

Mr. F. C. WEBB explained a modification of the ordinary sextant, by which larger angles could be measured than with the instruments now in use. Two sextants (by Messrs. Fletcher of Leadenhall Street,) were exhibited—one of the common arrangement, and the other on the modified principle.

It was stated, that when the arm of the common sextant was at zero, the lines of incident and of reflection of an object seen in the horizon-glass, formed a certain angle with one another, both at the object and horizon-glass; and that this angle, termed the constant angle, deducted from 180°, gave the extreme theoretical angle which could be measured. Practically, this measurement could be still further reduced, by the limits within which it was possible to reflect an object from a plane surface with accuracy, and which, if assumed at 170° instead of 180°, would give the angle from which the constant angle must be deducted to obtain the extreme angle capable of being measured. The smaller the constant, therefore, the nearer would this angle approach 170°, with a given amount of accuracy. It was observed that this constant angle was dependent on the relative position of the object-glass, the eye-piece, and the horizon-glass, and was, in fact, the angle formed by a line drawn from the eye-piece to the centre of the horizon-glass, with a line drawn from the centre of the horizon-glass to the centre of the object-glass.

The reduction of this constant angle to a minimum was effected, in the modification alluded to, by placing the eye-piece very near to the object-glass, and the horizon-glass as far as possible both from the object-glass and the eye-piece. The extreme angle capable of being measured was thus considerably increased, and with conditions more favorable to accuracy; for whilst the angles of incidence and reflection in the object-glass were not smaller than in an ordinary sextant, those in the horizon-glass were constantly larger. With a given angle to be measured, the conditions were more favorable to accuracy, since the

lines of incidence and reflection formed larger angles with the reflecting surfaces, both in the object and the horizon-glasses.

In marine and land surveying, and in taking altitudes of celestial objects with an artificial horizon (where the angle to be measured was double the altitude), this increase in the capabilities of the instrument was, it was believed, an advantage.

Proceedings Insti. Civil Engineers, May 20, 1862.

Cleaning of Platinum.

From the London Chemical News, No. 51.

SIR—A remarkably rapid and perfect method of cleaning platinum apparatus consists in gently rubbing upon the dirty metal a small lump of sodium-amalgam. Sodium has the curious property of lending to mercury the power of "wetting" platinum in so complete a manner that the positive capillarity between platinum and an amalgam containing even only a few per cent. of sodium appears to be as great as that between mercury and zinc, with this important difference, however—in the former case the "wetted" metal does not suffer the least trace of amalgamation. Even when foreign metals, such as lead, tin, zinc, silver, are purposely added to the sodium-amalgam, the platinum surface suffers no disintegration.

When the amalgam has been rubbed on with a cloth, until the whole surface is brilliantly metallic, water is applied, which oxidizes the sodium, and allows the cohesion of the mercury to assert itself. On wiping the mercury off, the platinum surface is left in admirable condition for the burnisher. I suppose the sodium to act here chiefly as a diluent, diminishing thereby the cohesion of the mercury, and allowing the adhesion between that metal and the platinum to predominate, a result which is certainly assisted by the mercury enabling the sodium to offer a clean surface to the platinum, and so allowing the specific adhesion between the two latter metals to be exhibited.

I am, &c. F. G.

Laboratory, University of Edinburgh.

Removal of Nitric Acid Stains from the Hands. By M. SCHWARZ.

From the Lond. Chemical News, No. 53.

We know that nitric acid stains the skin yellow, and that it is impossible to remove the discoloration by ordinary re-agents. We know also, that these stains disappear only when the epidermis is renewed. M. Schwarz advises for their removal the use of sulphide of ammonium, with the addition of a little caustic potash. By this means the coloring matter is not destroyed, but the burnt epidermis is converted into a soapy substance, which can be scratched off with a small piece of wood, the nail, or rubbed off with sand. By washing with a little dilute sulphuric acid, the skin becomes clean and recovers its natural whiteness. M. Schwarz believes that in some cases the above combination might be used as a caustic, and that its application might prove serviceable in certain affections of the skin.—*Rép. de Chimie.*

A Lecture by Dr. W. Odling, F.R.S., *on* Mr. Graham's *Researches in Dialysis.* Delivered at the Royal Institution Feb. 14, 1862.

From the London Chemical News, No. 118.

Before proceeding to unfold to you in regular order the subject which I have undertaken to bring under your notice this evening, I wish to direct your attention for a few moments to one or two experiments which, as some little time is required for their performance, I will now set in operation, so that they may be ready for me to refer to at a later part of my address. The instrument which I hold in my hand is called a Dialyser. It is constructed somewhat like a tambourine. It consists of a piece of membrane, or vellum, or parchment paper, stretched tightly over a hoop of gutta-percha, so as to form a sort of tray capable of holding water, and capable of being floated upon water. In each of these jars is a dialyser of similar construction to the one in my hand, but somewhat smaller. We have in this bottle a quantity of the red coloring matter obtained from coal tar, known as Magenta; and I am about to pour a little of it into one of these dialysers. [A small portion of the magenta was floated in a dialyser.] Now, I have here another red coloring matter, though a much less brilliant one; nevertheless it is possessed of very considerable tinctorial power, as I can show you by pouring a small quantity of it into a glass of water. This is the coloring matter of blood. You perceive that a very little of it is sufficient to impart a considerable degree of coloration to a large body of liquid. Now, I will pour some of this other red coloring matter—the coloring matter of blood—into this second dialyser—[referring to one floating in another jar.] I have here a brown coloring matter, caramel, obtained by the roasting, or rather, heating of sugar. Its color, I suppose, will be tolerably apparent at a distance. I will mix some of this brown coloring matter with the magenta, and pour the two into the dialyser floating on the water in this tall jar [referring to a third]. We will allow the whole to remain undisturbed some little time, and at a later period of the lecture return to our examination of the jars in order to see whether or not any effects have taken place.

And now, by way of introduction to the proper subject of my story this evening, I will recall to your recollection that intermixture, or diffusion, which takes place when two different liquids are in contact with one another. I have here a tube bent in the shape of the letter U, such a one as is ordinarily known in laboratories as a U-tube. Into one limb I have already introduced a weak solution of common salt, which, for the sake of greater distinctness, I have colored pale blue. Into the other limb I now pour a solution of another salt—namely, Epsom salts, or sulphate of magnesia, of about equal strength, which, for the sake of distinctness, I have colored red. We have now in one limb of the tube a weak solution of common salt, colored blue, and in the other limb of the tube a weak solution of Epsom salts, colored red. The question is, whether these two salts will remain separate, as they now are, or whether they will mix with one another. We find,

as a matter of observation, that after a certain time a mixture does take place. A slow diffusion of the one into the other occurs; the common salt passes into the sulphate of magnesia limb, and the sulphate of magnesia passes into the common salt limb, until a perfect uniformity of composition in the liquid is established. I may show you the experiment in a somewhat different manner. Here, instead of taking a double-limbed tube, I take a double-bulbed tube. The lower bulb is filled with a solution of common salt, colored pale blue. I will now pour in some water; and here we shall have to consider the same question as before. Will the heavy common salt remain at the bottom of the tube, or will it gradually rise up into the light water? Here, as in the other instance, repeated observation tells us that the heavy common salt will not remain at the bottom of the tube, as, by its superior gravity, we might expect it would, but that it will, in opposition to the law of gravity, so far as that alone is concerned, rise up through the light water. Now, this experiment, although a very well known one, is, nevertheless, of considerable interest. We have to inquire why the common salt, which is heavy, should not remain at the bottom, instead of rising up to the top through the light water. We admit generally that, in order to set a body in motion, the action of some external force is required. Now, I would ask, what is the external force which causes the heavy common salt to rise up through the light water? I have poured in this water somewhat roughly, and thereby have created a greater disturbance of the liquids than I intended, so that some of the blue solution of common salt has, by a mere accidental shaking, got into the upper bulb; but, quite irrespective of that shaking, a quantity of the common salt would gradually rise into the upper bulb and into the stem above it.

The rising up of the common salt into the water is termed its diffusion. The phenomena of liquid diffusion were first examined some 15 or 16 years ago by Mr. Graham, who has, with more or less of interruption, continued his researches upon the subject down to the present time; and it is to some of his results that I propose to direct your attention this evening.

His first experiments were conducted by means of a process which he termed "vial diffusion." The substance was allowed to diffuse from a small vial or cylinder. The experiment was conducted somewhat in this manner:—I have here two glass jars, one within the other. The internal jar is filled nearly to the top with a solution of the chloride of copper—the salt whose diffusiveness is to be made the subject of experiment. I may here direct your attention to a similar experiment which is going on in the actual kind of apparatus used by Mr. Graham. In the internal cylinder or vial of this jar we are causing the diffusion to take place, not of the green salt, chloride of copper, but of the yellow salt, bichromate of potash. You see the interior vial containing the bichromate of potash solution, and the exterior jar the water. Well, the experiment was conducted in this manner. The interior cylinder was filled with the solution of the salt whose diffusiveness was to be ascertained; it was then introduced gradually into an

external jar of water, and the whole was set aside for some time. During that time a portion of the salt contained in the interior vial diffused itself out into the exterior liquid. The experiment was terminated by withdrawing the interior vial gradually, and then ascertaining, by evaporating down the external liquid, or by some other means, how much of the salt from the vial had got into the external water. This was the mode in which his original experiments were conducted. I may say, with regard to these two experiments which are in progress in the jars to which I have just referred, that they were arranged this morning about eleven o'clock, and you perceive that very little, if any, perceptible diffusion has taken place. Nevertheless, some of the salt has in each instance passed into the external liquid, as we might very readily prove.

Now, among the very many interesting results to which this very simple process gave origin, there are only two or three which have a direct bearing upon the subject of my lecture this evening, and of them I will now speak.

The first general conclusion arrived at was this:—that different salts differ very much from one another in the rapidity with which they diffuse. That is shown in this table labeled "Vial Diffusion." A series of these vials, perfectly similar to one another, having the same capacity, and having, more particularly, the same area of opening, were filled with the same quantities of solutions of the same strength of different substances; one with a 20 per cent. solution of common salt, one with a 20 per cent. solution of sulphate of magnesia, one with a 20 per cent. solution of nitrate of soda, and another with a 20 per cent. solution of gum. They were introduced respectively into jars of water of this size and character—possibly into this identical jar—and allowed to diffuse for eight days at the temperature of 60°. This table [pointing to the subjoined] gives some of the results obtained:—

Vial Diffusion.

Substance.	Diffusate.
Chloride of Sodium,	58·68
Sulphate of Magnesia,	27·42
Nitrate of Soda,	51·56
Sugar,	26·74
Gum,	13·24
Albumen,	3·08

It was found that from the 20 per cent. solution of chloride of sodium or common salt, 58·68, or rather more than 58½ grains, had passed out into the larger jar—had diffused in fact. Now, this quantity which had passed out into the external jar was spoken of by Mr. Graham as the *diffusate*. He found that from the 20 per cent. solution of sulphate of magnesia not quite 27½ grains had diffused; from the solution of sugar 26·74 grains had diffused; from the solution of gum 13 grains; and from that of the albumen only 3 grains.

Now, it is obvious from a mere inspection of this table that the

process of diffusion might be made available to effect, at any rate, a *partial* separation of substances—a partial separation, for instance, of a highly diffusive substance, like chloride of sodium, from a feebly diffusive substance, like albumen. Thus, if we were to take a solution containing equal weights of albumen and common salt, and were to pour it into a jar of water, the ratio of the albumen to the common salt would be the same in the dilute liquid resulting from the mixture of the water into which we pour it and the solution, as it would be in the original solution itself. But if, instead of pouring the solution into a mass of water, we were to pour it into one of these vials, and then allow it to diffuse into a mass of water, a very different result would take place: for every 58 grains of common salt that passed out there would be only 3 grains of albumen pass out into the external vessel, so that whilst in the liquid into which we poured the solution we should have the two compounds in the ratio of 1 to 1, or 100 to 100, in the liquid into which we allowed it to diffuse the ratio of common salt to albumen would be as 58½ to 3, or as 100 to about 5.

But even this is not all. Not only are we capable by diffusion of effecting a partial separation of bodies which are merely mixed together, but we are also capable of effecting the decomposition of definite chemical compounds. I have here a piece of alum, a remarkably definite and crystalline body. It is a double sulphate of the two bases potash and alumina. Now, if we make a solution of this alum, and pour it into one of these vials, and introduce the vial into a diffusion-jar and allow diffusion to take place, the tendency of the potash to diffuse being much greater than that of the alumina, the potash actually breaks away from the alumina with which it was in combination, in order to diffuse itself into the external water; so that at the termination of the experiment we find in this exterior liquid a certain quantity of free sulphate of potash, whereas in the internal vial we find a certain quantity of free sulphate of alumina, which sulphate of potash and sulphate of alumina have resulted from the chemical decomposition—that is, the breaking up of the chemical compound, alum, effected by the superior tendency which the potash had to diffuse over the tendency which the alumina had. And so in a great number of other instances, actual decomposition may be produced by the process of diffusion.

Mr. Graham's later series of experiments on diffusion were conducted in a somewhat different manner. A certain quantity of water was introduced into a jar, and the saline solution—the solution of salt, whose diffusiveness was to be ascertained—was then carefully conveyed to the bottom of the jar by means of a pipette. I have here the experiment on a somewhat large scale. We have constructed a pipette out of a separator, and we will now allow a quantity of common salt solution (which, for the sake of distinction, I have colored red) to pass down through the water. We are now conveying it to the bottom of the jar, where, you perceive, it forms a distinct layer. I have made this experiment on a somewhat larger scale than Mr. Graham employed, in order that it may be seen over the theatre. Here, however, we

have one of Mr. Graham's jars, in which an experiment was begun early this morning. It will be rather difficult for me to render it visible all over the theatre, but some of you will see that there is here at the bottom of the jar a distinct layer of yellow liquid, namely, a solution of bichromate of potash, which was introduced this morning in much the same manner as I am now introducing the red liquid. The salt so introduced is allowed to diffuse into the superincumbent water. The experiment is conducted for a certain number of hours or days, and at the end of that time the exterior liquid is very carefully drawn off from the exact top of the liquid by means of a syphon, in definite layers. For instance, in this mode of conducting the experiment, there were 700 cubic centimetres of the liquid, and 100 cubic centimetres of the solution of the salt to be diffused. After the diffusion had gone on for some days, the top 700 centimetres of liquid were drawn off in portions not of 100 but of 50 centimetres, so as to make 14 portions or layers of 50 cubic centimetres each; a double layer of 100 centimetres being left at the bottom. The amount of salt which had diffused into each layer was then ascertained by evaporating each one separately, or the amount of salt contained in each layer was determined in some other manner. Some of the results obtained in this way are given in the table. I should say that, for the sake of distinguishing between the two processes, Mr. Graham designated this one (that of the former experiment) as vial diffusion; while this (the experiment last described) he called jar diffusion. These, then, are some of the results of jar diffusion:—

Jar Diffusion for Fourteen Days.

Stratum.	Common Salt.	Epsom Salts.	Albumen.
1	·104	·007	
2	·129	·011	
3	·162	·018	
4	·198	·027	
5	·267	·049	
6	·340	·085	
7	·429	·133	
8	·535	·218	·010
9	·654	·331	·015
10	·766	·499	·047
11	·881	·730	·113
12	·991	1·022	·343
13	1·090	1·383	·855
14	1·187	1·803	1·892
15 and 16	2·266	3·684	6·725

Let us consider the results obtained with a solution of common salt. Originally there were 10 grammes of common salt introduced into the 100 centimetres of liquid which occupied the lowest layers. In the top layer we now find only $\frac{1}{10}$th of a gramme of salt; in the 5th layer we fine nearly $\frac{2}{10}$ths; in the 7th layer $\frac{3}{10}$ths; in the 8th layer $\frac{4}{10}$ths; in the 12th layer $\frac{9}{10}$ths; in the 13th layer $\frac{9}{10}$ths; and in the 14th layer $\frac{1}{10}$ths of a gramme of salt. The two original layers, the 15th

and 16th, were not separated, but examined together, and in them were found $\frac{2}{16}$ths of a gramme of the salt. Now, if we compare this diffusion of common salt with the diffusion of albumen which took place under precisely the same conditions, we find that, whereas there was a considerable quantity of common salt in the topmost stratum of the liquid into which that substance had diffused, the albumen in the same time did not rise higher than the eighth stratum; and whereas in this case we introduced ten grammes of common salt into the lowest hundred cubic centimetres of liquid, and found that out of those 10 grammes $7\frac{3}{4}$ had passed out; in the case of the albumen we likewise introduced 10 grammes into the lowest 100 cubic centimetres, and found that less than 3 grammes had diffused into the superior strata. Perhaps these results will be rendered more evident by means of this diagram, which represents sections of the two jars. In the case of the common salt the whole of the two lower layers were at first occupied by the solution of common salt: at the end of the experiment we find that the common salt has actually risen up to the very top of the liquid, and could have risen considerably higher, as is shown by the fact that the quantity of common salt contained in the top layer of liquid into which it diffused is 20 times as great as the quantity of sulphate of magnesia contained in the top layer of liquid into which it diffused under precisely the same conditions. The common salt then not only rose to the top, but it could have risen higher, whereas the albumen rose only to the top of the eighth stratum. And whereas in the one case we have little more than 2 grammes of the common salt left in the lowest two layers, in the other case we have more than 7 grammes of the albumen, showing the great difference in the diffusive nature of salt and albumen. Of all the bodies in this table, salt is the most highly diffusive; then sugar and sulphate of magnesia, which are much alike; gum, still less diffusive, and tannin much the same as gum; albumen, again, is still less; and caramel still less than albumen. Now, these results of jar diffusion bear out generally those of vial diffusion. We are capable of obtaining the same separation of salts from one another, and although my time is getting on very rapidly, I must for one moment direct your attention to the actual separation that was in this way effected. This larger table of the jar diffusion of mixed salts refers to an experiment in which a 5 per cent. solution of common salt was mixed with a 5 per cent. solution of the analogous chloride of potassium; and the two salts were allowed to diffuse together for a period of 7 days at the temperature of 55°. Now, it was known that chloride of potassium diffused more rapidly than chloride of sodium. The results obtained by vial diffusion had shown that they diffused in about the proportion of 10 to rather more than 8. Now, the uppermost 6 strata of the liquid, into which the diffusion took place, were found to contain, altogether, at the end of 7 days, 561 milligrammes of the mixed salts, and out of that 561 milligrammes, 404 were chloride of potassium; so that these 6 upper layers contained, altogether, 72 per cent. of chloride of potassium. At the beginning of the experiment, the liquid contained of chloride of potassium and chloride of sodium

in the proportion of 50 to 50; but in these top 6 layers we obtained a mixture of the two salts in the proportion of 72 per cent. of the one, and 28 per cent. of the other.

Now, this [pointing to another table] is a similar experiment, conducted with the same base, but with different acids. Sulphate of soda and chloride of sodium were mixed in solution and allowed to diffuse for 7 days at a temperature of 55°, and with these results. I should say that the difference in diffusibility between the sulphate of soda and the chloride of sodium is very much greater than that between chloride of potassium and chloride of sodium, being in the proportion of 10 to 7, instead of 10 to 8. The first 6 strata contained altogether 263 milligrammes of mixed salts, and of those 263 milligrammes, 239 were common salt, and the remainder only was sulphate of soda or Glauber's salt; so that nearly 91 per cent. of the mixed salt contained in the 6 topmost layers was chloride of sodium, and only the remaining 9 per cent. was sulphate of soda.

(To be Continued.)

Valuable Substitute for Metal.

From the Lond. Mining Journal, No. 1389.

Some two years since we mentioned the ingenious application of adamas as a substitute for metal in the manufacture of gas burners, and stated that the same substance was equally applicable to various other purposes for which metal was then employed. The use of the "adamas" burners has since that time become very general, and Mr. Leoni, the inventor and manufacturer of them, has now succeeded in introducing adamas taps and adamas machine-bearings, the working of which has given the greatest satisfaction to those who have employed them. The mode of manufacture consists in reducing the silicate of magnesia to an impalpable powder, and then moulding it into the desired form, and annealing it, the result being that with the greatest facility the utmost precision may be obtained. When employed for taps, the advantage is that an article is produced upon which neither heat nor acids have any effect, at a merely nominal price, and it is anticipated that at no distant period "adamas" steam cocks will come into general use, to which purpose the material is undoubtedly well adapted, since upon a trial of a couple of ordinary adamas beer-taps (the price of which will be but 1*s.* or 1*s.* 3*d.* to the retail customer), the one began to leak at a pressure of 65 lbs. to the inch, and the other stood upwards of 80 lbs. without being affected. But the purpose to which the metal may be considered as more especially applicable, is for the manufacture of machine bearings, the test which it has stood in this direction being certainly all that could be desired. A steel spindle was run in an adamas bearing for 100 entire days consecutively, at a speed of about 15,000 revolutions per minute, yet neither the spindle nor the bearing show the slightest appearance of wear, and several other experimental tests have proved equally satisfactory. But as a single practical application is preferable to any

amount of experimental testing, we may state that at the works of Mr. H. Grisell, the well-known engineer, a bearing has been for some time in use, and appears to succeed completely. They use it as a fan bearing as a substitute for a Babbitt's patent white metal bearing, brass having been previously proved to be quite inapplicable, owing to the great friction and resulting heat, and, although the shaft makes nearly 1000 revolutions per minute, it is found that the "adamas" bearing remains quite cool, requires oiling but once a day, and shows no appreciable signs of wear. In the position in question the life of a Babbitt's bearing is five weeks, and it is confidently believed that the adamas will last for more than as many months. Mr. Leoni intends to place a machine showing the working of the bearings, with shafts and spindles, making from 250 to 15,000 revolutions per minute, in the forthcoming International Exhibition, and we have no doubt that the value of the discovery will be appreciated by all users of steam-power, whether in the shape of mining, marine, or industrial engines. For the bearings to railway carriages it would also be especially adapted, owing to the non-liability of adamas to become heated, and we hope ere long to be enabled to record the result of its practical application to this purpose.

The Ravages of the Limnoria Terebrans on Creosoted Timber. By DAVID STEVENSON, F.R.S.E., M.I.C.E.

From the Lond. Civ. Eng. and Arch. Jour., July, 1862.

The following is an abstract of a very useful paper recently read before the Royal Society of London:—

The author stated that it would be difficult to estimate the value of any chemical or mechanical process whereby timber might be rendered permanently impervious to the ravages of *Limnoria terebrans*, that small but sure destroyer of timber structures exposed to the action of the sea.

The ravages of that crustacean were first observed in 1810 by Mr. Robert Stevenson, the engineer of the Bell Rock Lighthouse, in the timber supports of the temporary beacon used by him in the erection of that work. Having forwarded specimens of the insect, and of the timber it had destroyed, to Dr. Leach, the eminent naturalist of the British Museum, Dr. Leach, in 1811, announced it as a "new and highly interesting species which had been sent to him by his friend Robert Stevenson, civil engineer," and assigned to it the name of *Limnoria terebrans* (Linnean Trans., vol. xi. p. 370; and Edinb. Ency., vol. vii. p. 433).

The *Teredo navalis*, which was a larger and even more destructive enemy, was happily not so prevalent in northern seas as the Limnoria.

Experiments made at the Bell Rock by Mr. Robert Stevenson, extending over a period of nearly thirty years, the detailed account of which was given in Mr. Thomas Stevenson's article on Harbors in the "Encyclopædia Britannica," had clearly proved that teak, African oak, English and American oak, mahogany, beech, ash, elm, and the

different varieties of pine, were found sooner or later to become a prey to the Limnoria. Greenheart oak was alone found to withstand their attacks; and even this timber was said in some instances to have failed.

Mr. Stevenson's experiments also included the testing of the artificial processes of Kyan and Pain, the former being an injection of corrosive sublimate, and the latter of proto-sulphate of iron. Timber prepared by Kyan's process was attacked in two years and four months, and in four years and seven months was quite destroyed. Timber prepared by Pain's process was attacked in ten months, and destroyed in one year and ten months.

The justly approved creosote process, patented by Mr. Bethell, had been largely employed in railway works, with universally admitted success; and, in common with many of his professional brethren, the author adopted it in several marine works, in the expectation that it would prove an antidote to the Limnoria; but having now ascertained beyond all doubt that creosote was not a universal or permanent preservative of timber used in marine works, the author proposed in the present notice to state briefly the facts on which this opinion was grounded.

Before doing so, however, he wished it to be distinctly understood that he did not undervalue Mr. Bethell's highly important invention as a preservative of timber against all ordinary decay incident to railway sleepers, timber viaducts, and indeed all timber structures not exposed to sea-water infested with the *Limnoria terebrans*. His remarks referred exclusively to its application for marine works below half-tide level. For all other classes of works he believed it to be a most valuble preservative.

In 1859, in a discussion which followed a paper on the "Permanent Way of the Madras Railway," at the Institution of Civil Engineers, the author first stated that there were distinct evidences of the attack of the *Limnoria terebrans* on creosoted timber used at Scrabster Harbor, in Caithness: while Mr. Bethell the patentee, and others, expressed their conviction that creosoted timber could not be perforated by any worm or insect.

Subsequent experience and observation have satisfied the author that the statement which he then made was correct; the fact, as now ascertained, being that thoroughly creosoted timber is, in certain situations, readily perforated by the *Limnoria terebrans*.

The first instance to which he referred was the pier at Leith, which was executed about 1850, by the late Mr. Rendel. The whole of the timber employed was creosoted on the spot in the most careful manner. As the piers at Leith were washed by a constant admixture of fresh water from the Water of Leith, the author expected that the progress of devastation at that place would be so slow as to be hardly appreciable on creosoted timber. But having carefully examined the West Pier, he corroborated the evidence given by Mr. A. M. Rendel in 1860, before the Select Committee on Leith Docks Bill, that notwithstanding the most careful application of creosote, the timber work has been attacked by the insect to a great extent.

The second case to which he referred was Invergordon. Two steamboat jetties were constructed at that place from designs by Messrs. Stevenson. It was generally represented that there were little or no traces of marine insects in the Cromarty Frith, and it was resolved that it was a situation peculiarly suitable for employing timber pile-work protected by creosote. The timber used in the work was carefully selected at Leith, and dressed to the necessary scantlings and lengths, so as to avoid all cutting after it had undergone the process of creosoting. It was then creosoted by an agent sent by Mr. Bethell for the purpose, at the sight of a careful inspector employed by the engineers. Every piece of timber was weighed before being put into the tank, and the process of creosoting was continued until each piece had received, as nearly as possible, the specified quantity of 10 lbs. of oil per cubic foot. Some experimental pieces were from time to time cut longitudinally, when it was found that the creosote had entered the ends of the logs 18 inches to 2 feet, and that it had saturated the timber some two or more inches all round. No greater precautions could possibly be used to insure perfection in carrying out the process, which involved an additional cost of about £450. The jetties were erected in 1858, and now the superintendent's report was, "that the blackened or creosoted portion of the timber is very much eaten and perforated. The timber perforated is just as it came from the creosoting tank, never having been cut. There is 1¼ inches wasted on some of the piles that have been perforated."

The third case to which he referred was Scrabster, which was also constructed under Messrs. Stevenson's directions. The timber employed in this instance was selected Memel of first rate quality; it was carefully creosoted at Glasgow. On cutting up a timber that had been attacked by the Limnoria, it was found that the creosote had fully entered at the ends and saturated the sides, and yet it was discovered to have been attacked after it had been exposed only 13 months—the insect perforating the blackened timber. The whole of the creosoted portion of the timber work was now more or less worm-eaten and destroyed. Mr. Leslie had also directed the author's attention to similar results at Granton and Stranraer, at both of which places the creosoted timber had been perforated.

The author held that these instances were enough to prove that the failure was not peculiar to one spot or one isolated case. If it was said that the timber used at these places had not been properly creosoted, it might fairly be concluded, that if the process, even when conducted in the patentee's own works, to the satisfaction of careful inspectors, was so difficult and uncertain in its results, its general applicability would be greatly injured. All newly creosoted timber, whether it was well or ill done, presented the same appearance externally; and it was only by weight that the completeness of the saturation could be judged of; and if careful weighing before and after the timber had been creosoted was not to be held as an ample and satisfactory test that the process had been properly conducted, it seemed hopeless to expect that perfect satisfaction could be attained. But it was so far

fortunate for Mr. Bethell's system, that it was not needful in the cases to which allusion had been made to call in question the extent of saturation which his process secured when properly executed. The timber at Scrabster and Invergordon, and he believed at the other places named, was undoubtedly thoroughly and properly saturated; and the author said that the explanation of the failure was to be found in the fact, that the Limnoria *perforated timber which had been thoroughly creosoted and blackened*—a fact which at once disproved the assumption, hitherto so generally made, that the poisonous nature of the creosote would prevent the insect from attacking it. As the Pholas perforated stone to procure shelter, the Limnoria might excavate timber for the same purpose, and obtain its food from the minute animalculæ with which the water of the ocean was charged. Dr. Coldstream, in his elaborate paper on the Limnoria in the "Edinburgh New Philosophical Journal" for April, 1834, had concluded that the Limnoria fed on the timber, and not on animal substances; but even if this were so, there seemed no reason to conclude that creosoted timber could not be eaten by insects, on account of the poisonous nature of the preparation employed. The author stated, that it had been ascertained that there were insects that lived and fattened on food that was to man a deadly poison. In the "British Medical Journal" for April, 1862, there was an interesting notice on the subject. Mr. Attfield had there shown that substances which are intensely poisonous to the higher animals do not affect Acari, which he found not only readily ate, but actually fattened on strychnine, morphine, and other deadly poisons. But the author stated that the specimens which he had laid before the Society proved conclusively that creosote does not act as a poison in preserving the timber, because it could be seen that the Limnoria were embedded in wood still highly charged with creosote.

After carefully considering the subject, the author had no doubt that the process of creosoting preserved timber from the attack of marine insects only so long as the oil existed as a film or coating on the outside of the timber. Whenever the attrition caused by the motion of the sea removed this outer film or coating, and exposed the fibrous surface of the timber, the insect would then attack and perforate it, whether it were creosoted or not, its search being for a fibrous substance in which to burrow. The time that might elapse before the timber became assailable to these insects depended on the situation. Wherever there was little abraiding action of the sea, the exterior film of creosote might be longer preserved; and where there was a considerable admixture of fresh water to check the growth, or at least the avidity of the insect, the effect of their ravages might be more gradual, or, in some situations, almost inappreciable. But the result of the author's observation and experience led him irresistibly to the conclusion, that on the northern shores of the country, where works are exposed to the open sea, creosoted timber was readily perforated by the Limnoria, and could not be safely employed in any important part of a marine structure at or below half-tide level, a fact of great importance to the civil engineer.

Strength of Iron in Construction.

From the London Builder, No. 1011.

In a recent paper on the Properties of Iron, more especially with regard to ship-building, read by Mr. Fairbairn at the Royal Institution, some remarks occurred which may be usefully printed.

The lecturer said as to

The Properties of Iron.—If we are desirous to attain perfection in mechanical, architectural, or ship-building construction, it is essential that the engineer or architect should make himself thoroughly acquainted with the properties of the materials which he employs. It is unimportant whether the construction be a house, a ship, or a bridge. We must possess correct ideas of the strength, proportion, and combination of the parts before we can arrive at satisfactory results; and to effect these objects the naval architect should be conversant with the following facts relating to the resisting powers of malleable and rolled iron to a tensile strain.

The resistance in tons per square inch of—

Yorkshire iron is	24·50 tons.
Derbyshire "	20·25 "
Shropshire "	22·50 "
Staffordshire "	20·00 "

Strength of Riveted Joints.—The architect, having fortified himself with the above facts, will be better able to carry out a judicious distribution of the frames, ribs, and plates of an iron ship, so as to meet the various strains to which it may be subjected; and ultimately to arrive at a distribution where the whole in combination presents uniformity of resistance to repeated strains, and the various changes it has to encounter in actual service.

There is, however, another circumstance of deep importance to the naval architect; which should on no account be lost sight of; and that is the comparative values of the riveted joints of plates to the plates themselves. These, according to experiment, give the following results:—

Taking the cohesive strength of the plate at	100
The strength of the double-riveted joint was found to be	70
And the single-riveted joint	56

These proportions apply with great force to vessels requiring close riveting, such as ships and boilers that must be water-tight; and in calculation it is necessary to make allowances in that ratio.

The next question for consideration is the properties of iron best calculated to resist the penetration of shot at high velocities; and in this I am fortunate in having before me the experiments of the committee on iron plates, which may be enumerated as under:—

Specific Gravity.	Tensile strength in tons per square inch.	Compression per unit of length in tons.	Statical resistance to punching in tons; 1 in. plate.
7·7621	24·802	14·203	49·1804

Remarks.—The specimens subjected to compression gradually

squeezed down to one-half their original height: increasing at the same time in diameter till they attained ninety tons on the square inch.

In these experiments four descriptions of iron were selected, marked A, B, C, D: the two first and last were taken from rolled and hammered iron plates; excepting C, which was homogeneous, and gave higher results to tension and dead pressure than the others.

In density and tenacity they stood as follows:—

Mark on Plates.	Density.	Tenacity in tons.	Remarks.
A Plates, . .	7·8083	24·644	
B Plates, .	7·7035	23·354	
C Plates, homogeneous,	7·9042	27·032	
D Plates, . .	7·6322	24·171	

Here, it will be observed, that the strengths are in the ratio of the densities, excepting only the B plates, which deviate from that law.

On the resistance to compression it will be seen that in none of the experiments was the specimen actually crushed; but they evidently gave way at a pressure of 13 tons to 14 tons per square inch, and were considerably cracked and reduced in height by increased pressure.

For the Journal of the Franklin Institute.

"*Gigantic Iron Casting.*"

In the July Number of the *Journal of the Franklin Institute*, page 58, is an article headed "Gigantic Iron Casting," extracted from the *London Mechanics Magazine*, January, 1862, giving an account of a huge anvil block, weighing from 35 to 38 tons, cast by Fawcett, Prestor & Co., Liverpool, for a steam hammer, to melt which two air furnaces and two cupolas were engaged; the quantity of iron melted being forty tons. The time required to cast, from opening the furnaces, being less than a quarter of an hour.

This was a heavy undertaking, and doubtless the "large number of ladies and gentlemen who assembled to witness the operation of casting, were highly gratified at the beautiful spectacle."

In a mechanical point of view, the casting of a similar piece at the Port Richmond Iron Works, I. P. Morris, Towne & Co., on July 1st, shows a much greater capacity for doing this description of heavy work.

The block weighs 31 tons, and to pour it 37½ tons of metal were melted in *one cupola* in four hours from the time the blast was put on. The quantity of air used in the blast was 4000 cubic feet per minute. The coal consumed was one pound for each nine pounds of iron melted.

The mould was filled in 4½ minutes pouring. The work was done with the ordinary resources of the establishment, and without any display or assemblage of visitors. W.

For the Journal of the Franklin Institute.

Remarks on Force, Motion, and Inertia. By JOHN C. TRAUTWINE, C. E., Philadelphia.

(Continued from page 120.*)

Although we speak of applying extraneous force to a body, we, in fact, usually apply it only to a certain *point* on the exterior of the body. At this point the force gradually enters, as I conceive, *unresistedly* into the body; and diffuses itself equally throughout it. This unresisted entrance appears to me to be proved by facts; but apart from them I cannot imagine it. We see that magnetic force passes unresistedly through wood, stone, most metals, glass, &c., and attracts a piece of iron on the opposite side, with as much intensity as if no obstacle were interposed.

The process of the equal diffusion of force, which consists (if I may be allowed the expression) in the substitution of the element matter, for the element velocity, (which two elements compose motion,) require *some* time, even in the densest bodies: and still more in porous and elastic ones. As force requires time to move a body from one place to another, so it requires time to diffuse itself throughout the different points of a body. If we strike a piece of iron, our force rapidly diffuses itself, and the whole piece appears to move at the very instant the blow is given. There is, however, a short interval, although not appreciable to our sight. But, if we strike a mass of cotton, the striking body will pass some very appreciable distance into it, before the moving force becomes equally diffused. The particles of cotton at the point to which the blow was applied, will be moving rapidly forward, before those most distant from it will have any motion communicated to them. Nay, the blow may be so rapid that the striking body may pass entirely *through* the mass, without communicating motion to the distant particles at all. This, I conceive to be, in consequence of want of *time;* or, which amounts to the same thing, to want of strength in the particles (adjacent to the moving body,) to *transmit* so much motion *at once*. Thus we saw, in the case of the spring-balance experiment, that the index of the balance could be made to point to several pounds at the instant of *transmitting* great moving force to an *unresisting* weight of but 1 pound. Now, the string by which the 1 pound was suspended from the balance, would have broken, had it

* I must correct two entirely inadvertent slips of the pen which occur in the first part of this communication; and which escaped my notice until after the number of the Journal containing them was issued.

On page 117, in the first two sentences commencing with "If a ball," &c., read respectively, "a re-action of 20 foot-pounds," and "a re-action of 1 foot-pound;" and omit the remainder of both sentences. Although forces re-act only as they become static, we have, I believe, no means of comparing the intensities of moving forces with those of static ones.

On page 119, fourth line from the top, strike out the words "Morin leading off." I have not seen the no-motion, no-friction theory in print; but it is entertained by many persons, including some of my personal friends of high scientific attainments. One of these, on my asking him why then a body did not slide down an inclination less than the so-called angle of friction, replied that its *inertia* prevented it from so-doing. If so, I presume that the inertia of matter is intimately connected with its *smoothness:* and may be modified by *oiling*.

My edition of Silliman's "First Principles of Philosophy," is of the year 1859; but a friend has within a few days shown me a later edition (1861), in which the expressions to which I have objected in this communication, are omitted. I had quoted from Silliman, because his book deservedly ranks among the very first of our elementary works on Natural Philosophy.

not been strong enough to resist the strain indicated by the balance; and in that case, only a portion of our moving force would have been communicated to the 1 pound weight; while the remainder would still have continued in our hand.

This strain produced in the medium through which moving force is *transmitted* to an *unresisting* body, or from one part of a body to another, appears to me to be a result of the *mysterious* action of force to which I have alluded. To persons accustomed to investigations of such subjects, the whole matter may be very plain; but to me it is at present, entirely incomprehensible.

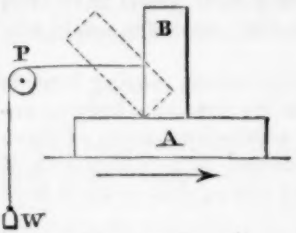
To render the source of my difficulty more clear, I will adduce another example. If in the Attwood machine, the small body which gives motion to the two large ones, be suspended from one of the latter by a string, then the string will be strained by the moving force which it transmits from the small weight to the large ones; and yet I cannot imagine that the large ones *resist* the moving force thus transmitted. If the string be not strong enough to sustain the weight of the small body, it will break, instead of transmitting the moving force. If the string be but *very little* stronger than enough to transmit the mere weight of the small body at the moment of starting from the top of the machine, it will safely transmit the far greater moving force accumulated in the small body during its descent, because time is allowed it during its descent, to do so by degrees, as fast as additional motion is imparted by gravity. But if it were attempted to pass this accumulated force through the string at once, it would break; and no more moving force would be transmitted through it to the larger bodies, than the amount required to effect the breaking. Either the string must be stronger, or more time must be allowed. The fact that a bar of iron may sustain a suspended weight for days or even months before being broken by it, shows that time is required for force to act upon the several particles composing that body.

The ordinary coupling between a locomotive and a heavy train, would break under the action of an engine capable of imparting to the train, *at one impulse*, a velocity of 40 miles an hour; yet it safely transmits the same amount of moving force when imparted by a succession of milder impulses.

So far as I can see, we have to admit one of two things; either that there is in *matter* a mysterious innate force called inertia, which *resists* moving force; or that *moving force* will, *of itself*, sever mediums through which we may attempt to *transmit* too much of it to *unresisting matter*, as well as to *resisting force*. The Attwood machine appears to me to *prove conclusively* that no moving force is lost, or in other words, *resisted*, or re-acted against; and that consequently there is no inertia. The same machine may be made to show that weak mediums for the transmission of moving force to unresisting matter, will break.

From want of time, cog-wheels break, when *suddenly* thrown into gear; and to the same cause are ascribable many other phenomena usually attributed to inertia.

The experiment which Morin adduces in proof that inertia is a resisting force is as follows:



Let A be a block, lying on a table; and B another block, standing loosely on A. Now if we suddenly give to A a rapid push in the direction of the arrow, B will fall backwards, as shown by the dotted lines. Or, if we begin to move A very slowly, in the same direction; and *gradually* communicate the rapid motion to it, so as not to upset B; and then suddenly stop the motion of A, B will fly forwards, and will upset in the direction opposite to what it did before. Morin says that B falls backwards in the first case from the resistance which its inertia opposes to motion; and in the second case, from the resistance of its inertia to rest. He adds, that if the block B be symmetrical, and we ascertain what weight, W, (suspended from a string passing over a pulley, P, and attached at its other end to the block B,) will overturn B, then it is evident that the energy of the inertia of B is at least equal to that of the weight; because it produces at least as great an effect as that weight.

Now it appears to me that when in the first case, we suddenly give A its rapid motion in the direction of the arrow, B falls backwards because we apply the moving force to A alone; and there is no medium at the joint between A and B, except the insufficient one of friction, for *transmitting* a rapid moving force from A to B. From the insufficiency of this medium, time is not allowed for equally substituting the matter of B, for the velocity of A. There is barely time for the lower part of B, which rests immediately on A, to receive some share of the velocity of the latter, before said lower part of B, in company with A, shall have moved away from under the top of B, so that B falls backwards.

I believe the reader will not deny that so rapid a movement can be imparted to A, that there will not be time to communicate more than the merest trifle of motion even to the lower part of B; and that A may be absolutely knocked away from under B so quickly, that B will be left standing upright on the table; especially if the joint between B and A, be lubricated so as to diminish the value of the medium, *friction*.

Now, according to the inertia theory, moving force is in this case actually transferred from A to B; but the inertia of B resists, or re-acts against said moving force, and thus enables B to stand still; whereas my idea is that B stands still, simply because the joint between it and A, is too inefficient a medium to transmit the rapid motion of A to B; or, in other words, that B being inert matter, does not move because we have not given it moving force. It appears to me that the inertia theory requires us to assume that the lubrication of the joint increases the inertia of B; inasmuch as B moves less when the joint is lubricated, than when it is not.

Again, if we apply motion to A, by instalments so small that they

may severally be transmitted through the joint, and have time to become equally diffused throughout B, until they shall suffice to produce rapid motion in B, without upsetting it; and if then we suddenly stop A, B will fly forwards, not in consequence of inertia, but in consequence of the moving force which we have put into it. There is no sufficient medium at the joint between A and B, by which this moving force of B can be transmitted back to A at one effort; and of course B must move forwards until it meets with some re-acting force which shall stop it. Had B been glued to A, the cohesion of the glue might have afforded this re-acting force; or had the glue been too weak to stop B entirely, it would at least have transmitted from B to A, the amount of force which was required to sever the glue. B upsets in both cases from the same cause.

So with persons in a carriage. If the horses start off suddenly, they impart rapid moving force to the *carriage*. The friction of a passenger against his seat, and that of his feet against the floor, serve as mediums for transferring the moving force of the carriage to those parts of his body. But if his head is not leaning against the back of the carriage, then the carriage, and said other parts of his body, will be moved forwards rapidly, before the elastic medium of the upper part of his body has had time to transfer motion to his head; which consequently will remain comparatively at rest, until the back of the carriage comes up to it, and by a violent thump compels it to move forward with the body to which it is attached. The head is not thrown backward against the back of the carriage, but the back of the carriage is moved forwards against the back of the head.

If the carriage stops suddenly, the passengers will be still carried forward, not by inertia, but by the moving force which has been accumulated in them; unless they avail themselves of some medium sufficiently strong to transmit said moving force back again to the carriage. This they generally do by grasping the hand-straps of the carriage itself. If the straps happen to be too weak for transmitting so much force all at once, they will at least transmit the amount which was necessary to break them; and the remainder will probably be rendered static by the striking of the body against the front of the carriage.

It appears to me that if there were a force inherent in matter, such as could enable it to resist even the slightest moving force, then there would be a certain slope within the limits of which water would not "seek its level." Or suppose a pair of scales, *absolutely devoid of friction*, and that in the scale-pans were placed equal masses of matter, each as heavy as our earth. Would not a single grain of sand added to either mass, communicate motion to both, in precisely the ratio existing between their united weights, and the weight of the grain of sand? Where and how great then is the force of inertia in the two earths; from what is its existence at all inferred; or what ratio does it bear to the weight of the grain of sand?

In conclusion, Mr. Editor, although aware that your pages are kindly offered to those who desire through them to seek for information, I cannot but feel sensible that I have trespassed too far on your indul-

gence in this instance. I feel a strong conviction that the views which I have expressed are to some extent, if not altogether, erroneous. The eminent authors whom I have quoted as entertaining other opinions, are perhaps sufficiently conclusive evidence that such is the case. Still it is plain that even they differ materially from one another; and "what shall scholars do, when masters disagree?" Not conceiving it to fall within the province of the merely practical engineer, to attempt the investigation of principles so specially appertaining to abstract science, I have not pretended to do so; indeed, I do not possess the acquirements necessary for that purpose; but have contented myself by endeavoring to show the difficulty under which I labor in attaining such an insight into the subject, as will inspire me with confidence in practical cases; and hence have allowed no false shame to deter me from exposing an ignorance which I see no better means of removing.

The questions which I should like to have answered are these:

1st. If when a body in motion strikes a body at rest, action and re-action are equal and opposite, where do we find the two equal, and *opposite* forces, which thus act and re-act against each other; and how much of the motion of each is lost thereby? Or can there be re-action, in the strict sense of the word, without absolute *destruction* of motion? If so, define re-action.

2d. Inasmuch as it is certain that the so-called inertia of matter does not resist, or re-act against *all* of any moving force which may be applied to it; against what *per centage* of such force does it re-act? and cannot that be ascertained by first finding what the quantity of motion in the weights of an Attwood machine should be, in case there were no resisting inertia; and then observing what quantity they actually have? Would not the difference be the loss arising from the re-action of inertia?

These questions involve the entire source of my difficulty, in comprehending the matter. I have of course, had reference, in most cases, to *inelastic* bodies.

For the Journal of the Franklin Institute.

Decision of the Hon. JAMES DUNLOP, *Judge of the Circuit Court of the District of Columbia, in the matter of the Appeal of* JEPTHA DYSON, in his Re-issue case, relating to Carding Engines, from the Decision of the Commissioner of Patents, September 21, 1860.—Reported by H. HOWSON, Esq., Philadelphia.

Ex-parte—JEPTHA DYSON'S Appeal.

This is an appeal to me by Jeptha Dyson from the decision of the Commissioner of Patents of date 7th July, 1860, refusing him a re-issue of his patent of the 20th February, 1849, for improvements in carding engines, with amended specifications and claims.

The gist of Mr. Dyson's invention is the differential motion of the stripper A, introduced upon the engine to clear the main cylinder C of the cotton imbedded in it in the process of carding, without stopping

the machine, by this self-acting contrivance. His original specification described only the fast motion of the stripper A, which fast motion was of a surface speed, exceeding the surface speed of the main cylinder C, the effect of which was to make A a clearer of C. It did not describe the slow motion of the stripper A, reducing its surface speed below the surface speed of the main cylinder "C" by means of a loose pulley on the shaft of A, the effect of which slow or differential motion of A at intervals made "C" a clearer of A, and enabled A again to resume its function of clearing the main cylinder "C," and thus to keep the engine in constant working order.

Mr. Dyson's original patent is inoperative and invalid as a self-acting contrivance to clear the main cylinder, by his failure to describe this differential motion of A. Without it A cannot be itself cleared when clogged with embedded cotton in it, and so cannot perform its function as a clearer or stripper of C. Mr. Dyson has sworn that his omission to describe in his specification this differential motion was by accident, mistake, or inadvertence, and without any fraudulent or deceptive intention, and that he designed originally to patent it in February, 1849; and he has proved by four witnesses that, before the original patent was applied for, they saw Mr. Dyson's engine worked with this differential motion, and that it was never worked otherwise.

The Office has rejected Mr. Dyson's re-issue application because the original specification, model, and drawing, do not, nor does either of them, show the differential motion; and they refuse to look at any evidence outside the record, as they call it.

They refuse to receive any proof other than the original record, however plenary it may be, to show this differential motion to be the same invention, intended to be patented by him in 1849, although the original specification and claim, in asserting a self-acting contrivance, does point to some other mode of clearing "C" than is set forth therein.

I agree with the Office, it is too obscure and vague alone, and without further proof in aid of it, to be the basis for inserting in the re-issue the differential motion; it is at most a circumstance to uphold and fortify the aliunde proof or the evidence of the witness outside the record.

Can such outside proof, if plenary and credible, sustain the re-issue? This depends upon the true interpretation of the thirteenth section of the Act of 1836.

The question is not free from difficulty, as will be apparent when I state it has been decided differently by two able and distinguished ex-Commissioners of the Office. I refer to the case of Jeremiah Carhart, decided by Judge Mason in 1856, and to the case of Adriance, assignee of Gale, decided in 1858 by Judge Holt. I have given to the subject the most careful and anxious consideration, and will state the reasons which have controlled my judgment. They have satisfied my own mind, and if they fail to satisfy others whose rights may be thereby compromitted, I have the consolation to know that these reasons

may be reviewed, and if wrong reversed before the proper judicial tribunals. My judgment can only give to Mr. Dyson a prima facie title to his re-issue, which is still open to contest in the courts by those who have a standing in them to dispute its validity.

The thirteenth section of the Act of 1836, or so much of it as relates to this case, is in these words: "That whenever any patent which has heretofore been granted, or which shall hereafter be granted, shall be inoperative or invalid by reason of a defective or insufficient description or specification, if the error has or shall have arisen by inadvertency, accident, or mistake, and without any fraudulent or deceptive intention, it shall be lawful for the Commissioner, upon the surrender to him of such patent, and the payment of the further duty of fifteen dollars, to cause a new patent to be issued to the said inventor for the same invention for the residue of the period then unexpired for which the original patent was granted, in accordance with the patentee's corrected description and specification, &c., &c.," (providing for assignees, and legal representatives;) and "the patent so re-issued, together with the corrected description and specifications, shall have the same effect and operation in law on the trial of all actions hereafter commenced for causes subsequently accruing, as though the same had been originally filed in such corrected form before the issuing of the original patent."

The first remark I make upon this section is, that as the Commissioner is a public officer, and the power conferred on him in this section concerns others (patentees), and is beneficial to them to have executed the words in the section, "it shall be lawful for the Commissioner, upon the surrender to him of such patent, &c., to cause a new patent, &c., to be issued, &c.," are to be construed as mandatory, and to be of the same import as if the words had been, it shall be the duty of the Commissioner, that is to say, the true meaning is, that the Commissioner is to have no discretion in the case provided for in the section.

When the case provided for arises, he is commanded to exercise the power, whether he thinks it just and right to exercise it or not in the case assumed in the section to exist, he has no discretion. For this principle of law I refer to the case of Mason et al. *v.* Fearson, 9th Howard, 249. In that case the Supreme Court say, "Whenever it is provided that a corporation or officer 'may' act in a certain way, or it shall be lawful for them to act in a certain way, it may be insisted on as a duty for them to act so, if the matter as here is devolved on a public officer and relates to the public or third persons."

The next remark I make upon this section is, that by its terms, when the case of honest mistake arises on a defective or insufficient description or specification, the only limitation on the re-issue patentee for his amendment or correction is, that it shall be for the same invention. The closest inspection of the section will show no other limitation.

By the terms of the section no mode of proof is pointed out to show the invention claimed on re-issue to be the "same invention;"

this is left at large; there is no prohibition of any particular species or class of evidence. In the absence of such prohibition, how can any legal evidence to establish the invention, to be the same invention, be excluded?

By legal evidence I mean all such evidence as by the rules of law and the adjudication of the courts is receivable, to establish any like fact in controversy before them.

In the case supposed of honest mistake, the section gives to the re-issue applicant an absolute vested right to his amendment, dependent solely upon the condition that his amendment shall cover the same invention originally intended to be patented. As he is not limited in the section to prove this fact by any specially prescribed testimony, how can it be said the Legislature did not intend the whole range of legal proof should be open to him? How can the Office rightfully undertake to say he shall only prove it by their record; that is, only by the specification, model, or drawings? That no evidence, however plenary and credible by competent witnesses, will be listened to or looked at.

Cases, it seem to me, may be put in which this inexorable rule would not only work great injustice to individuals, but in fact repeal this re-issue section.

The patent laws provide for "inventions" which do not require models and drawings.

Models and drawings are only required by law where they illustrate the invention claimed, and are thought necessary by the Commissioner.

"Compositions of matter" are by law patentable; in such cases there are no drawings and models; they are not needed; all that is required is a specification. The merit and usefulness of the "invention" consists (we may say for illustration) in the admixture of certain ingredients in certain proportions compounded in a particular manner. If an honest inventor, inadvertently and by mistake, errs in his original specification as to the ingredients, the proportions, or the mode of admixture, is he to be without remedy by re-issue? By the Office rule he would be.

There is no model or drawing to amend by; they were not required nor necessary. He cannot amend by the "specification;" this is defective and good for nothing; it is the very thing he wants to make good by amendment, and which the 13th section gives him the right to amend.

The Office rule excludes him from relief, because he cannot prove his case by the record, and the re-issue section is virtually repealed by the Office.

Again, put the case of a "machine" where a model and drawings are necessary, and have been deposited in the Office according to law. Put the case of the Carding Machine now in controversy. It does not properly belong to the model and drawings, to show the rate of speed at which the cylinders are driven in that machine. That is the appropriate function of the "specification." The merit of the invention in this case, consists in that rate of speed. The inventor swears, that by

mistake, he failed accurately to describe the speed, or rather the differential speed, of one of the cylinders which he intended to patent in 1849, and proves by four witnesses that they knew the machine before the date of the patent, and before the patent was applied for by Mr. Dyson, to be worked with this differential motion of the stripper A, and to be only so worked. Is the inventor, in such a case, to be deprived of relief by re-issue?

The Office rule excludes him, because their record does not show any thing to amend by, because it does not show the thing to be amended.

It was not, in this case, the function of the model and drawings to show the error, and they are therefore, as to this point, as if they did not exist. The only other part of the Office record by which the inventor could amend is the "specification," and that is the document he avers to be mistakenly wrong, and which the re-issue section was intended to enable him to put right. If that document is wrong, it cannot be put right by itself; and the model and the drawings being out of the way, as not the appropriate means to furnish the evidence, such evidence can only be got aliunde, or outside the Office record. If this is not received, there is a failure of justice.

Again, put the case in which the model and drawings are necessary, and in which their proper function is to show the whole "invention" and every part of it. If the honest inventor, by mistake, fails to show his entire invention, either by the specification, model, or drawings, is he to be without remedy? The Office rule says he is; but even there, which is the strongest case against an inventor, the statute does not in terms exclude him.

I agree, in such a case where all the three elements of proof which ought to serve him fail, it is strong, if not pregnant evidence, of a fraudulent and deceptive intention; but after all, even in that case it is only a question of the weight of evidence. It may, I think, under the statute, be met and rebutted by plenary and credible proof outside the record.

The Commissioner, in the case of Adriance, assignee of Gale, seems to think that on a re-issue there is a material difference between a defective and insufficient description and no description at all, and that it is the former only that the 13th section, truly construed, allows to be corrected.

If the Commissioner is right in this construction, then if there was no description in the "specification" of the real invention, although the drawings and model fully set it forth, the applicant would be without remedy; but the Office does not now, and I believe never did, press the rule to that extent; and the Commissioner's reason for this construction of the statute is, that the introduction by re-issue of features or devices not described at all or shown in the drawings or model, "would destroy the identity of the invention as patented," and hence the new patent would not, in the strictness of language, be for the "same invention as the old, which the law requires it shall be."

Now, I cannot see that a defective and insufficient description is any

better than none. Neither of them is any foundation for an operative patent. If it does not cover the invention the patentee seeks and secure his rights, it is worthless, and no better than no description. This construction seems to rest on verbal criticism merely.

Nor is the reason given, sound and tenable.

The reason is, that new features introduced would destroy the identity of the invention patented, and would not, in strictness of language, be for the same invention.

Now it is very clear, that the identity of the invention patented is always destroyed by a re-issue. The sole object of a re-issue, as intended by the applicant and provided for by law, is to give to the patentee something additional, though it be the same invention; that is to say, something not in the old patent; and to this extent every re-issue destroys the identity of the patented invention. There would be no sense or use in the re-issue, unless it added something to what was already patented. If it left the old patent intact, or identical as to invention, the patentee would gain nothing by his application, and the re-issue statute so construed, would fail to effect any good to the patentee, and might as well be blotted from the statute book.

What the Legislature designed to secure to patentees by this 13th section, was to enable them to cure honest mistakes, and to get substantially protection for the same invention they had made and intended to be patented when the original defective patent was granted. The only limitation in this statute is that the invention should be the same. The Legislature has not said by what proof the applicant shall show his invention claimed on re-issue to be the same invention made and intended to be got on his original application. He is not limited by the statute to prove it by the specification, model or drawings, any legal proof to show it to be the same invention, whether found in the record or aliunde, ought to be received and weighed by the Office.

No authority by law is given to the Office to limit the range of the applicant's proof, if it is such as upon the law of evidence, is held sufficient to prove facts before other legal tribunals. The Commissioner of Patents, by the 12th section of the Act of 1839, has "Power to make all such regulations in respect to the taking of evidence to be used in contested cases before him as may be just and reasonable."

But the Office has no right to make new rules of evidence, or to make rules against law, or to divest vested rights by its rules of practice. No such power exists in any Court, not even the Supreme Court. The rules of practice of all courts are made in subordination to law, and no court in this country, so far as I know, can change, abrogate, or limit the known rules of evidence.

If these rules of evidence work injustice and lead to fraud and perjury, Congress must change them and not the Office. But I see no reason to think that fraud and perjury are more likely to be practiced on a re-issue than on an original application.

Where an invention is valuable, and large gains to be made, fraud and perjury are apt to be practised as they are in all other human

transactions outside the Office where great profit is to be attained.—Fraudulent inventors must be met as all other fraudulent or perjured actors are, by the ordinary legal remedies. The public and the members of it injured, must prove and expose the fraud and perjury and punish the transgressors.

Because this is often difficult to be done, innocent inventors ought not to suffer, who seek to correct errors, the result only of honest mistake, inadvertence, or accident, more especially when such right of correction is expressly conferred by statute.

It is said the Office may be deceived and made the innocent instrument of deception, by granting patents for the like invention to subsequent claimants. When this is so, the Office has only to declare an interference and then the truth can be brought out, at all events the subsequent patentee can then bring his adversary face to face, and cross-examine his witnesses and offer evidence on his own part to expose any attempted fraud or perjury.

The 26th Rule of the Office is referred to in the case of Adriance, assignee of Gale, to justify the exclusion of any other evidence on a re-issue than is furnished by the "Record," that is to say the specification, model, or drawings. The rule is in these words, or the portion of it relied on. "A specification cannot be amended in any material part unless there is something to amend by, that is to say, it can only be so amended to cause it to correspond with the drawing or model." But this rule applies only to original applications, and is classed, and appears under that head. It is in furtherance of justice, and to secure the revenue of the Office and not contrary to the statute.

An original applicant has no right by law to an amendment unless by the 7th section of the Act of 1836, after his first rejection, to conform his specification to the alterations suggested by the Commissioner.

It is in the discretion of the Office in all other cases to grant the amendment or not, and when granted to impose such terms as are just. The legal right of the rejected original applicant is not to amend except in the case above alluded to, but to withdraw his application, leaving ten dollars in the treasury to pay the Office for the trouble.

If this rule did not exist, the original rejected applicant, on the old fee of thirty dollars, might by amendment make an entirely new case as often as he saw fit, to the annoyance of the Office, and in evasion of its rightful revenue.

Not so in the case of a re-issue. The amendment there is not of grace but of right. It is secured by the statute. The applicant is to pay fifteen dollars towards the revenue, and is limited to the "same invention" intended to have been embraced in the original defective patent. When these conditions are complied with, his right to the amendment is perfect under the law. This 26th rule therefore does not apply to re-issues. It would be void if it did, because contrary to law, and the failure of the Office so to apply it, contradicts, rather than sustains the construction of the 13th section now insisted upon. The 44th rule of the Office which does relate to re-issues, was not re-

lied on by the Commissioner who decided the case of Adriance, assignee of Gale, although it is relied on by the present Commissioner in his answer to the reasons of appeal. That rule is in these words: "The general rule is, that whatever is really embraced in the original invention and so described, or shown that it might have been embraced in the original patent, may be the subject of a re-issue." This rule is very cautious and general in its terms, and properly so. It does not profess to be without exception. It is only a general rule. It states what may be the subject of a re-issue. It does not say what shall not be.

It states one mode of showing the invention to be the same invention, and even this in vague and ambiguous terms, but does not prescribe that this shall be the sole and only mode. It leaves special cases, as I construe it, open to be determined by the law, applied to their particular circumstances. At all events it is not in the bold and peremptory terms of the rule of practice laid down by the Board of Appeals in their report in this case.

The counsel who has argued this case for Dyson, who was himself long a Commissioner, insists that the practice of the Office has been the reverse of that alleged in the report of the Appeal Board. He refers to Conillard or Quillard's case in 1838, Woodworth's Planing Machine case in 1845, Pond's case in 1847, letter book 345, and in 1856 to Jeremiah Carhart's case decided by himself. After all, however, the case must be decided not by the practice of the Office, but by the law applicable to it. The practice and decisions would seem to have been variant under different Commissioners.

The liberal spirit in which the Patent Laws ought to be construed in favor of honest patentees, is so strongly set forth by Judge Marshall in the case of Grant et al. *v.* Raymond, in 6 Peters, 241, 242, that I will cite passages from that opinion of the Supreme Court.

Before giving the extracts it is proper to say, that the decision was made in January Term, 1832, more than four years before the act of 4th July, 1836, and that in that case they sustained the re-issued patent, even though the Secretary of State who granted it, had then no express power by law to grant a re-issue in cases of accident, mistake, or inadvertence on the part of the patentee.

Judge Marshall said: "If the new patent can be sustained it must be on the general spirit and object of the law, not on its letter—to promote the progress of the useful arts is the interest and policy of every enlightened government," &c.

"It cannot be doubted that the settled purpose of the United States has ever been and continues to be, to confer on the authors of useful inventions an exclusive right in their inventions, for the time mentioned in their patent. It is the reward stipulated for the advantages derived by the public for the exertions of the individual, and is intended as a stimulus to those exertions. The laws which are passed to give effect to this purpose ought, we think, to be construed in the spirit in which they have been made, and to execute the contract fair-

ly on the part of the United States," &c. "That sense of justice and of right which all feel, pleads strongly against depriving the inventor of the compensation thus solemnly promised, because he has committed an inadvertent or innocent mistake." "It has been said that this permission to issue a new patent on a reformed specification, when the first was defective, through the mistake of the patentee, would change the whole character of the act of Congress. We are not convinced of this: The great object and intention of the Act is to secure to the public the advantage to be derived from the discoveries of individuals, and the means it employs are the compensation made to those individuals for the time and labor devoted to these discoveries, by the exclusive right to make, use, and sell, the things discovered, for a limited time. That which gives complete effect to this object and intention, by employing the same means for the correction of inadvertent error which are directed in the first instance, cannot, we think, be a departure from the spirit and character of the Act," &c.

"It has been urged that the public was put into possession of the machine by the open sale and use of it, under the defective specification, and cannot be deprived of it by the grant of a new patent—the machine is no longer the subject of a patent. This would be perfectly true if the second patent could be considered as independent of the first, but it is in no respect so considered—the communication of the discovery to the public has been made in pursuance of law, with the intent to exercise a privilege which is the consideration paid by the public for the future use of the machine. If, by an innocent mistake, the instrument introduced to secure this privilege fails in its object, the public ought not to avail itself of this mistake and to appropriate the discovery without paying the stipulated consideration. The attempt would be disreputable in an individual," &c., &c.—and a fortiori, in the Government of the United States.

The Supreme Court in the case before them, enlarged the Patent Law by construction, to give effect to its spirit, and to hold the United States to the fair execution of their agreement with the patentee.

The Patent Office in the case of Dyson, before it, with the express power of re-issue conferred by the 13th section of the Act of 1836, restrains by construction both its letter and spirit, and denies the re-issue unless the patentee proves his case by the original record, the inadvertent and honest errors of which record he wants to put right, and which it was the object of the 13th section to enable him to put right. The model and drawings may, and often do, furnish the means to correct the defective description; not so always; the differential motion (as it is called) of the stripper A, is in this case the very gist of the invention; it does not belong properly to the model and drawings to show this differential motion, or the rate of surface speed of any of the cylinders in the engine; this is appropriately the office of the specification. Mr. Dyson does not, as I understand, on this application for re-issue, propose to modify his model or his drawings.—He presents the same model and the same drawings which were offered and received at the Office on his original application, and thinks

them sufficient for the re-issue. It is true, the Board of Appeals in their report say, that the model does not show the loose pulley on the shaft of the cylinder A, but if it was there it would not necessarily show the differential motion the reduction by the slow pulley of the surface speed of the stripper A, below the surface speed of the main cylinder "C," at intervals so as to make "C," a clearer or stripper of A. The loose pulley is one of the means, and only one, to effect this differential motion. Dyson does not claim a patent for the means, but for the motion; the means are not new, but familiar to every mechanic. If the fast and loose pulley were on the shaft of the cylinder A, without specifying the rate of speed to be effected by the loose pulley, it could not be known but that the loose pulley was designed to stop the cylinder, one of the usual offices, I am told, of the loose pulley.

But the 13th section does not in the terms point to the model and drawings as the sole means of proof, or any means of proof; the whole matter of proof is left at large. It requires that the invention sought to be introduced in the amended description, should be the same invention originally intended to be patented, and is silent as to how that is to be ascertained.

The spirit of the section as well as its letter is to give to the patentee the invention originally intended; and the Supreme Court say the United States ought in good faith to confer this on him as they have promised to do.

He is to prove it to be the same invention intended, but the *quo modo* of proof is not defined, and of course it is open to the patentee to offer any sufficient legal proof, record, or otherwise. If he is confined to record proof alone, the Office, by construction, restricts both the letter and spirit of the re-issue clause of the act of Congress.

I refer to the cases of Brittin et al. *v.* Taggert et al., 17 Howard, 83, and Allen *v.* Blunt et al., 3 Story, 744, as throwing light on this question.

In the last mentioned case Judge Story says: "Whether the invention claimed in the original patent, and that claimed in the new amended patent is substantially the same, is, and must be, in many cases, a matter of great nicety and difficult to decide. It may involve considerations of fact as well as of law."

If the Office rule of practice is right, as laid down in the report of the Board of Appeals in this case, I do not see how there can be any dispute about "fact." The record, the Office says, is conclusive, and no other proof can be received or heard. It would seem Judge Story had in his view aliunde testimony.

For the reasons given in this opinion, I think the appellant has sustained his first and second reasons of appeal; and I do, this 21st September, 1860, reverse the judgment of the Commissioner of date the 7th July, 1860.

If there be no claimant or subsequent unexpired patent for the same invention claimed on this re-issue by Mr. Dyson, then I think Jeptha Dyson, the appellant, is entitled, on the proof, to his re-issue as claimed by him; but if there be such claimant or subsequent unexpired

patent, then I think an interference ought to be declared, and the parties litigant heard on proof before the Office.

I return all the papers, model, and drawings, to the Hon. the Commissioner of Patents, with this my opinion and judgment, this 21st September, 1860. JAS. DUNLOP.

To the Hon. PHILIP F. THOMAS,
Commissioner of Patents, Washington.

NOTE.—Where the words "slow pulley" occur, they are used the same as loose pulley.

The Production of Photographs by Steam.

From the Lond. Mechanics' Magazine, Oct., 1860.

The following paper, read recently by Mr. G. H. BABCOCK, before the American Photographical Society, has been sent us for publication, and will be accepted by readers as a significant indication of American progress:—

In this age of steam, telegraphs, and photography, when the three most subtle agencies of nature—light, heat, and electricity—have been subdued by man, and trained to do his bidding, startling developments and astounding applications in art and science are looked for as an almost daily programme in the great drama. Is it then to be wondered at that the go-ahead Yankee, in his impatience at the slowness of this "fast" age, should conceive and actually carry out the idea of applying steam power to the production of photographs, and should turn them out at a speed which eclipses the boasted rapidity of the "lightning" printing press?

As an evidence that this may be and even has been accomplished, I have the pleasure of presenting for the inspection of the Society, this evening, several specimens, among them a sheet containing about three hundred photographs, all printed from one negative, at the rate of *twelve thousand an hour!* As astonishing as this speed may seem, I am assured, and from what I have seen, believe that it may be greatly increased.

The means by which this is accomplished is, simply the adaptation of machinery to the process of printing by development. This process, though little used of late, has certainly produced some very fine specimens, and prints so produced are generally conceded to have the advantage in permanency over the ordinary prints produced by the direct action of light.

The machine is the invention of Mr. Charles Fontayne, of Cincinnati, Ohio, who has spent several years in perfecting it, and the developing process which he uses therewith. I am assured by him that the process by which these specimens were produced is quite different from any other known; but in what this difference consists I am not informed. A negative is fixed in a box, together with a sheet of prepared paper, and the latter exposed by automatic machinery to the condensed light of the sun passing through the negative. After each exposure the paper is traversed underneath the negative, to present a

fresh surface for the succeeding impression. These motions, together with that of clamping the negative into close contact with the paper at the instant of exposure, are all performed by the operator simply turning a crank.

The rapidity, at the several times I witnessed its operation, was two hundred impressions per minute, at which speed the time of exposure was but ·03 of a second for each impression. The condensing lens being seven inches in diameter, and the circle of condensed light about one and a half inch, the above exposure is equal to ·65 of a second direct exposure to the light of the sun. If, therefore, the machine were to be used for a larger class of pictures, such as book illustrations and stereograms, a condensing lens might be dispensed with, and yet nearly 2500 impressions be taken in an hour.

This opens a field for photography hitherto impracticable in consequence of the time and expense of printing, as ordinarily practised. The illustrations for a book, having all the exquisite beauty and perfection of the photograph, may be turned out, by the use of this machine, with a rapidity wholly undreamed of, either in plate printing or lithography. The expense of engraving may be dispensed with, and the negative come direct from the artist's hands, drawn upon a prepared glass, from which, in the course of a few hours, the plates for a large edition may be printed, each one a perfect duplicate of the original drawing. As an evidence of the facility with which this may be done, a print produced by the ordinary ammonia-nitrate process, from a rough sketch so prepared, is herewith presented. It will be seen that an ease, freedom, and spirit is given to the drawing which cannot be equalled by any process of engraving, and when the negative is properly prepared by an experienced artist, nothing further could be desired for illustrating ideal subjects; but for the actual, and for reproducing the works of others, of course, the draftsman would give way to the far more truthful camera.

But, besides book illustrations and portraits for visiting cards, and advertising purposes, of which specimens are shown, this machine may be applied to the multiplication of stereograms, which, by its use, may be made so cheaply as to bring them into the humblest family, where by their exquisite beauty and truthfulness they will engender a taste for the beautiful, and, in time, entirely eradicate the cheap and disgustingly coarse lithographs, engravings, and water-color daubs, which at present form so large a proportion of the pictures within reach of the poor.

When these new adaptations of photography shall have been fully accomplished, then shall our noble art, which has already done more to develop and elevate the taste of the present generation than any other one instrumentality, take a stand by the side of its great sister art—Printing—and hand in hand, will they go forth to educate, ennoble, and elevate mankind.

The pictures herewith presented possess additional interest to this Society, from the fact that they are taken upon ordinary American writing paper, which was not prepared specially for photographic pur-

poses. Mr. Fontayne first used this paper in his experiments on account of its cheapness, and, having become accustomed to it, he now prefers it to any of the foreign photographic papers. In the course of his extended experiments, he has used almost every variety of American paper, including that made from straw, manilla, and cane, with varying degrees of success, and he promises at some future time to present the result of these experiments to this Society.

Australian Coal.

From the Lond. Mechanics' Magazine, October, 1860.

The discovery and working of coal mines in New South Wales are very important facts, and have already had the effect of creating an active trade between Australia and Kurrachee for the supply of India. The following reports have been made upon the qualities of the coal by Mr. Anderson and Mr. F. A. Abel, both of Woolwich Arsenal:—

Report on Australian and other Coals by Mr. Anderson, Inspector of Machinery to the War Department.

On receiving instructions to test the Australian coal, I drew up a series of regulations for my own guidance, so as to secure a correct result and comparison.

The relative merits of Welsh and Hartley coal being the subject of controversy at the present time, I have combined these coals with the present experiment, and trust that the additional knowledge thus afforded may be found useful.

In carrying out these experiments a Cornish boiler has been employed, of a cylindrical form, and with a single internal flue, which contained the furnace.

To attain accuracy in the quantity of water evaporated, the boiler was carefully measured and graduated. Before each experiment began, water was pumped into the boiler up to a definite water-line, and no additional water was admitted until the conclusion of the experiments, when the exact quantity evaporated was carefully recorded.

The steam generated during each experiment was employed to work an exhausting engine with a constant and uniform load upon it; and, to insure accuracy in comparing the work done by different coals, an instrument was applied to the engine which unerringly recorded each revolution.

In each trial 2 cwts. of coal was used. Before commencing, I performed a series of preliminary experiments with English coal, so as to get the apparatus into perfect order.

Previous to commencing, the steam was brought to a standard pressure of 15 lbs.; then the former fire was drawn out of the furnace, the heat being sufficient to ignite the experimental fuel. When the fuel was exhausted, the cinders from under the bars were thrown upon the fire and burnt up. When the steam reached the standard pressure of 15 lbs., the experiment was considered at an end, and the water evaporated. Work done and ashes left were recorded. No steam was al-

lowed to pass off at the safety valve during an experiment, so that the result may be relied upon.

The following table shows at a glance the relative value of the several coals operated upon. It will thus be seen that for steam purposes the Australian coals are about equal to the Newcastle coal termed "Whitworth," which correspond with a considerable variety known as Walls-end, Lambton, Pelaw, Primrose, &c., but that it is slightly inferior to the Hartley and Welsh varieties of English coal.

The last column of the table shows exactly the relative value of the different experiments, and the true value of the several coals for steam engine purposes.

The Whitworth, Welsh, and Hartley coals were selected at random from the bulk used in the furnaces of the Royal Arsenal.

Table of Experiments with Australian and English Coal, made in the Royal Arsenal, Woolwich, March, 1858.

No. of Experiments.	Date.	Name of Coal.	Weight of Coal.	Duration of Experiment.	Weight of Ashes.	Water Evaporated in cubic Inches.	Weight of water evaporated by 11 lbs. of coal.	No. of revolutions made by steam engine.
			cwt.	h. m.	lbs.		lbs.	
1	March 18,	Australian,	2	5 20	13¼	45,780·5	7·4	3,950
2	" 18,	Do.	2	5 20	13¼	45,780·5	7·4	3,946
3	" 22,	Whitworth,	2	5 12	16	45,780·5	7·4	3,970
4	" 22,	Do.	2	5 10	15½	45,780·5	7·4	4,000
5	" 23,	Welsh,	2	5 30	5	50,621·5	8·6	4,367
6	" 23,	Do.	2	5 30	4¾	50,621·5	8·6	4,345
7	" 24,	Hartley,	2	5 25	3¼	49,636·5	8·0	4,290
8	" 24,	Do.	2	5 17	3½	48,654·0	7·8	4,199

March 29, 1858.

JOHN ANDERSON,
Inspector of Machinery.

Report on Australian Coal, by Mr. Abel, Chemist to the War Department.

The coal was found to contain a very considerable admixture of shale. Very thin seams of iron pyrites were observed to be interspersed through the massive portions of the coal itself.

An average sample of the coal as supplied was prepared, and the following points determined with it:—

1. The quantity of volatile matter furnished by the coal amounted to 71 per cent., and 29 per cent. of a light and very porous coke was obtained. The coal is, therefore, of a highly bituminous character, and would probably be found excellently adapted for the manufacture of gas.

2. The per centage of ash contained in this average sample of coal amounted to 6·8 per cent. The ash was of a red-brown color, due to the oxide of iron produced from the pyrites in the coal.

A massive piece of the coal was selected for a special determination of the ash, and was found to furnish 4·09 per cent.

The coal therefore yields, even when free from shale, a quantity of ash unusually large for a bituminous coal.

3. The per centage of sulphur found in the average sample amounted to 1·11, a quantity slightly exceeding that usually found in Newcastle coal, but which cannot be looked upon as considerable when compared with the large amount of ash furnished by this coal.

It would appear from the chemical examination of this coal that it is adapted for ordinary purposes, and for the manufacture of gas; but it will not be found equal in value to the description of coal especially employed for steam purposes.

F. A. ABEL,
Chemist to the War Department.

Recent Improvements in Lucifer Matches.

From the London Chemical News, No. 127.

Of matches prepared with ordinary phosphorus, and which consequently ignite readily upon any friction surface, the "Patent Paraffine Matches" of Messrs. Letchford & Co., are particularly good examples. Instead of the objectionable sulphur coating, melted paraffine is used for impregnating the wood and rendering it more inflammable. Such matches are not likely, therefore, to play havoc with the silver candlesticks and bright metallic surfaces often brought near them in actual service. Their power of remaining uninjured by damp is a special character for which this kind of match is remarkable; in a comparative examination of several different sorts, these only were capable of being ignited after six hours exposure to a moist atmosphere. On this account they would be particularly suitable for export, and little affected by climate.

Purifying Water from Lead.

From the London Mechanics' Magazine, October, 1860.

Some time since there appeared in the *Times* a letter from Prof. Faraday, explaining a very simple mode of treating water that was contaminated by receptacles of lead in the neighborhood of the sea (a matter of great interest at many military posts). Sir John Burgoyne having observed this letter, requested Dr. Faraday to favor him with a note on the subject, with the addition of any simple practical remedies, if such there were, for the presence of lead in water arising from other combinations. The following answer was returned by that eminent chemist:—

Royal Institution.

My Dear Sir John—I consider your request relating to the leaded water an honor, and in replying may add an observation or two to the original matter. The case at first was simply that of certain waters, which, having been collected from rain, by roofs, gutters, pipes, or cisterns of lead, were contaminated more or less with the metal. All water so obtained has not been found thus affected, and there is much difference and uncertainty about the mutual action of lead and water in different cases. When rain water falls upon surfaces of lead, it is

apt to act on them; and the water thus contaminated, by standing exposed to air, generally clears itself from the dissolved lead, the metal separating as a carbonated precipitate, and falling to the bottom. But when the sea spray has access to the leaded surfaces, the action of the rain water is such that the dissolved lead does not separate in this way, or if it does, only after a much longer time.* It is such water as this that I recommend to be treated with carbonate of lime. Enough whitening or levigated chalk is to be mixed with the fluid to make it of the consistency of good milk (though more will do no harm), and the whole is either to be filtered or to stand until clear. I have never yet found any sample of water poisoned as above, that was not freed from the lead by this process; and from the actions that occur in the laboratory, I have no doubt, that if two or three pounds of such powdered chalk were put into a cistern, and stirred up occasionally after rain, it would keep the water free from lead. Now, my consideration was entirely confined to cases of the above kind, and to the service of the Trinity House. I might say much more to you about the modes of testing for lead in water, so as to discover its presence, and, within certain limits, its proportion, and also about the clearance of lead from all domestic waters by filtration or otherwise; but I have always found that chemical practice was required to make such knowledge available, and that for that reason it was nearly useless in the hands of the public. When, too, a particular case becomes mixed up with the numerous cases that may be associated with it, I think it often disappears from view, and the whole are after a time forgotten. Hence, I prefer adhering to the case of adulteration arising from the joint action of salt water, or sea spray, and lead; and I have the full confidence that if it arise at any of your military posts at home or abroad, no difficulty will be found in the effective application of the remedy.

I am, my dear Sir John, your very faithful servant,

M. FARADAY.

To Sir J. F. BURGOYNE, &c., &c., &c.

* Professor Faraday, in his letter to the Editor of the *Times*, stated that "the salt of the sea spray, which often reaches the roofs of buildings, even when they are half-a-mile or more from the shore, causes the rain water to dissolve a portion of the lead, which is larger or smaller under different circumstances, and at times rises up to a quantity injurious to health, and poisonous."

Nitrate of Soda in the neighborhood of the River San Francisco, Bahia, Brazil.

From the London Chemical News, No. 89.

I have the honor to acknowledge the receipt of your lordship's despatch of January 7 last, instructing me to report, after a careful inquiry, on the localities in which nitrate of soda has been found in this province, its state of purity, the cost of extracting and refining it, and the expense of transport to a shipping port. In reply I beg to state, that ever since the mention made in my Commercial Report for the year 1856 of the discovery of this important manure in this province, I have received undoubted information of its existence in several other localities, and in consequence lost no time in renewing my exertions,

with the assistance of a few friends having connexions in the interior, to obtain every information possible thereon. The information received from several districts is such as to satisfy my most sanguine expectations, especially in the north-east of this province, in the neighborhood of the River San Francisco, at a place called "Salitre." In the words of a most distinguished and talented native of the town of Joazeiro, through a valley of 16 to 20 leagues long, one mass of nitrate of soda is found; in some places on the surface, at others a few feet under; and it contains sufficient manure for exportation for years to come. On the opposite side of the above-named river, near the town of Pilao-Arcado, other deposits exist in equal quantities. The projected Bahia railroad to the town of Joazeiro must pass through the first named district, but those magnificent deposits will only become available when that railroad reaches the San Francisco. In the interest of the Company and of commerce it is ardently to be desired that its works should be commenced and prosecuted with vigor, as the distance is only 240 miles from the city.—*Extract from the Report of H. B. M. Consul.*

Stoppers of Caustic Solution Bottles.

From the Lond. Chemical News, No. 89.

It is well known that the stoppers of these bottles rapidly become incrusted; greasing is of little use, and moreover, we have to consider that we may thus introduce fatty acids into the solution. Now paraffine answers well: first, because caustic alkali does not act upon it; and secondly, because it perfectly lubricates the surfaces in contact.

Polyt. Notizblatt.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, August 21, 1862.

John Agnew, Vice President, in the chair.

Isaac B. Garrigues, Recording Secretary.

The minutes of the last meeting were read and approved.

Letters were read from the Royal Society, London, the Liverpool Literary and Philosophical Society, Liverpool, England; and the Smithsonian Institution, Washington, D. C.

Donations to the Library were received from the Commissioners of Patents, the Royal Society, the Royal Astronomical Society, the Institution of Civil Engineers, the Chemical Society, the Institute of Actuaries, and the Society of Arts, London; the Osterreichischen Ingenieur-Vereines, and the Nieder-Osterreichischen Gewerbe-Vereines, Vienna, Austria; Prof. A. D. Bache, Messrs. F. Emmerick, and Charles Colné, Washington, D. C.; the Chamber of Commerce of the State of New York; the Legislature of Pennsylvania, Harrisburg, Penna.; and Mrs. Elizabeth Spohn, Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's statement of the receipts and payments for the month of July was read.

The Board of Managers and Standing Committees reported their minutes.

Candidates for membership in the Institute (3) were proposed, and the candidates (4) proposed at the last meeting were duly elected.

Mr. Howson, of the Committee on Meetings, exhibited a Military Cap, the invention of W. F. Warburton, of this city. The edge of the cap is provided with a number of eyelets or hooks, by which a detachable cap or havelock may be attached. A loose fold, forming a part of the cloth or covering of the cap, is arranged so as to conceal the hooks and eyes, the fold being retained in its position over them by elastic loops attached to the corners, and passing around the strap buttons.

Mr. Howson also exhibited an exceedingly well executed pair of Bronzes, manufactured by Messrs. Warner, Miskey, and Merrill, of this city. The designs are copies, but the composition of the metal and the casting are the work of the above-named firm, who were the first in this country to manufacture heavy bronze work, one of their finest pieces being the bronze balustrade for the Capitol at Washington.

Mr. H. also exhibited a Musket, which, he remarked, was to all appearances similar to the many weapons of this class we every day see in the hands of our soldiers. The specimen, however, would be looked upon with unusual interest when they were informed that it was one of the first complete regulation muskets made in this city, and a specimen of the first one thousand muskets recently delivered to the Government by the manufacturers and contractors, the well-known firm of A. Jenks & Son, of Bridesburg. It had fallen to the lot of the exhibitor to examine minutely many modern fire-arms, both of American and European manufacture, and he could state that, as regards excellence of workmanship, neatness of finish, and superiority of material employed, the weapon could not be excelled.

Mr. Howson also stated that few could imagine, without investigating the subject of manufacturing fire-arms, the extent of the enterprise undertaken by the Messrs. Jenks. In order that some idea of the amount of labor and thought demanded in establishing a manufactory such as that of Messrs. Jenks, Mr. Howson produced a small pistol, known as Sharps' four-shooter, and stated that a witness had recently sworn that to manufacture such a weapon profitably, an outlay of $100,000 was necessary.

On comparing the two weapons, the members could form some judgment of the enormous amount of capital required and the ingenuity and forethought exercised by the Messrs. Jenks in perfecting, during little more than one year, the machinery and apparatus required to manufacture the weapon exhibited, at a price remunerative to themselves, and with the rapidity which the present emergencies of the government demanded.

At the close of the meeting, the musket was carefully examined by the members, and attracted much attention.